

INQUIRY

Editor

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Vol. 3

1960



OSLO UNIVERSITY PRESS

© The Norwegian Research Council for Science and
the Humanities, 1960
(Norges almenvitenskapelige forskningsråd)

Printed in Norway by
A.S John Griegs Boktrykkeri — Bergen

CONTENTS

Articles

✓ BLACKSTONE, WILLIAM T.: Can Science Justify an Ethical Code?.....	118
FAIN, HASKELL and E. F. KAE LIN: Student Philosophical Opinions	137
HANDY, ROLLO: Doubts About Ordinary Language in Ethics	
HARRAH, DAVID: The Adequacy of Language.....	73
HINTIKKA, K. JAAKKO J.: Aristotle's Different Possibilities..	18
HUNGERLAND, ISABEL C.: Contextual Implication.....	211
KAE LIN, E. F. and HASKELL FAIN: Student Philosophical Opinions	137
KAUFMAN, ARNOLD S.: The Irresponsibility of American Social Scientists	102
MARGOLIS, JOSEPH: Actions and Ways of Failing.....	89
MATHIESEN, THOMAS: The Unanticipated Event and Astonishment	1
MONTAGUE, RICHARD: Logical Necessity, Physical Necessity, Ethics and Quantifiers	259
✓ RYNIN, DAVID: Statements, Components and Extensionality	153
✓ SHAPER E, DUPLEY: Philosophy and the Analysis of Language	29
WILSON, PATRICK: Austin on Knowing.....	49

Discussions

BULL, HEDLEY: Systematic Innovation and Social Philosophy.	199
HANNEBORG, KNUT: Should Poetry be Considered a Kind of Discourse?	128
STIGEN, ANFINN: What Does Mr. Tennessen Mean, and What Should I Say?.....	180
TENNESSEN, HERMAN: Vindication of the Humpty Dumpty Attitude Towards Language	185
WEIL, GUNTHER M.: Esotericism and the Double Awareness..	61
WEILER, GERSHON: Is Humpty Dumpty Vindicated?.....	

A New Member of the Advisory Board

We are glad to announce that Professor Gilbert Ryle, of Magdalen College, Oxford, has kindly consented to join the advisory board of *Inquiry*. Professor Gilbert Ryle has already been active once in the interests of our publication, suggesting the name "*Inquiry*" rather than the somewhat fancy Greek name Zetesis, proposed by

the Editor.

Oslo, January 1961

THE UNANTICIPATED EVENT AND ASTONISHMENT

by

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Introductory Abstract: Phenomena that are unanticipated or based on something unanticipated are often neglected by sociologists. 'Astonishment' is selected for analysis as one of the phenomena that are frequently based on unanticipated events. Especially when unanticipated events occur together with certain other social factors, astonishment is a likely reaction. Astonishment is further analysed in terms of some basic elements of social action: The reaction may be a *means* (especially of social control), it may be a conscious *end* in action, and it may be a *condition* in action.

I. Introduction

The concept of *expectation* has gained a rather secure place in the hierarchy of terms of sociologists. For many purposes of analysis it seems fruitful to distinguish between two basic types of expectations: 'normative' and 'anticipatory' ones. By 'normative expectations' is meant evaluative standards applied to a position or an individual, while 'anticipatory expectations' denotes statements, feelings etc. with regard to the probability of future events.¹ The two types of expectations are often, but by no means always, positively correlated.

Sociologists are not always clear with regard to what type they refer to when they talk about expectations. The former type certainly does seem to be the most usual one, but it is often more or less vaguely implied that in so far as social relations are normatively regulated, they are also 'expected' in the sense of being considered rather probable as future events.

As indicated already, this implication is often empirically tenable. However, it may be maintained that it has led to too much emphasis on

the regular, predictable, structured, anticipated aspects of social life, and to neglect of important phenomena that by their very nature are unstructured, unanticipated, or based on something unanticipated.²

The present paper may be viewed as a sociological study or analysis of some aspects of unanticipated phenomena. I will deal with one frequent and significant type of reaction to the occurrence of unanticipated events: *the state and overt signs of being astonished at such events*.

The analysis has a subjectivistic and not an objectivistic point of departure. Therefore, when we use such terms as 'the phenomenon of being astonished at something', they are shorthand descriptions of an allegedly subjectively felt inner state or emotion, or behaviour correlated with such an (allegedly) subjectively felt state. A strict objectivistic definition of astonishment does not interest us (though subjectivism in sociology can admittedly not be full or complete). — Moreover, our point of reference will be the person *who is astonished*, not the person who initiates the astonishing act. Unless otherwise specified, the concept of 'actor' will in fact refer to 'the astonished', not 'the astonisher'.

A person may be astonished, bewildered, amazed, surprised, perplexed, stunned, flabbergasted, puzzled, stupefied, electrified, dazzled, . . . Some of these terms may probably be distinguished from each other in terms of intensity, and they may perhaps be arranged on a continuum of strength from 'slightly puzzled' on the one pole to 'stunned' or 'stupefied' on the other. And other qualitative differences are no doubt possible between the terms. Yet, we will not be much concerned with linguistic differences. For the purposes of the present paper, 'astonishment' may be used as a *general* heading, covering almost all such terms. By such an imprecise procedure we probably lose a great deal. Yet, it is felt that we gain at least as much by being able to call attention to fairly general principles.

However, it may briefly be mentioned that when we refer to 'surprise', we do not use this term in the sense of being caught redhanded. Surprise in this sense is certainly a highly significant social phenomenon. For example, it is very relevant in relation to means of social control. Yet, we will not deal with this here.

The analysis that follows constitutes an inquiry neither into the rare, great crises in human life, nor into the affairs of the extraordinary person, but rather into the smaller, more common and obvious pains and pleasures of the ordinary man.

Now, first of all, what sociologically relevant conditions help produce the state or overt behavioural signs of astonishment?

II. Astonishment in relation to the unanticipated and to other factors

Astonishment as an inner state and as a consequent observable behaviour pattern may be positively associated with lack of knowledge of events in the environment, in so far as they may be associated with the unanticipated. Astonishment may then be viewed as a state or behaviour pattern that may result from acquiring new knowledge.

But what aspects of the 'unanticipated' are most relevant here?

When an event is *unanticipated*, it may be more or less unforeseen. And it may be unforeseen in at least two fundamentally different ways:

(1) The event may be unforeseen in the sense of being a contradiction of prior expectations about what is going to take place in the future.

(2) The event may be unforeseen in the sense of happening in an 'expectational void'; in the sense of having no prior expectations about what is going to happen in the future connected with it. Such an event is 'a bolt from the blue'. We will later come back to the basic importance of the difference between these two types of the unforeseen.

Furthermore, when an event is unanticipated, relevant actors may be more or less unprepared for it in the broad sense of 'not quite knowing what to do about it'. It is important to remember two different ways of 'not knowing what to do about' a given event.

(1) The event may pose the problem of the mysterious: The event is viewed as unexplainable, or at least unexplained. Here the actor 'does not know what to do about the event' in the sense of not knowing how to explain it. 'Explanation' is here taken in a very wide sense. It refers not only to rational cause-effect arguments, but also to arguments about habit, tradition, etc.

(2) The event may pose the problem of further action on the event; of controlling it in some way. This problem may clearly be associated with the problem of explanation. Yet it need not be.

Now, it seems that the mysterious may, under some conditions, be all that is needed to pull the trigger of astonishment. In other words, when the situation is ripe, the element of the unforeseen is not necessary. This may be reflected in the common saying "I get just as surprised at it every time." This statement implies that something occurs over and over again, and often that it is to some extent foreseen, but in so far as it is viewed as unexplainable or at least unexplained, it is still astonishing. Once and for all, we recognize the definite importance of the mysterious though foreseen.

In spite of this, the factor of the unforeseen (in either of the two senses mentioned above) will often be present when astonishment occurs, and it will certainly heighten the subjective feeling of being astonished. Since the combination of the unforeseen and unexplainable or unexplained so frequently is empirically relevant in the genesis of astonishment, we will concentrate on it in the present paper, in spite of the fact that some of the aspects of astonishment as treated below, could have been viewed in relation to the mysterious and foreseen. From now on 'unanticipated' will be used in *the limited sense of mysterious and unforeseen*. But it must be remembered that the constellation of the mysterious and unforeseen is not a necessary condition for the occurrence of astonishment (Though the element of the mysterious is perhaps a necessary condition").

Now, there are clearly many sociologically relevant factors that condition the occurrence of the unanticipated in the first place. The most significant one seems to be the factor of the institutional context. 'Institutional context' is here taken in the broad sense of normative structure related to specific functions. We may take as our point of departure a saying that often relates to the field of technology: "We do not get surprised at all." In technology, we are used to sudden and rapid changes; we have, so to speak, become disenchanted. In other contexts, such as within religious or family relations, even slight exceptions from the usual may astonish if not horrify us greatly.

Now, the relevance of the institutional context clearly hinges on the importance of attitudes toward *the new*. Whereas the new in some institutions is unacceptable and even resisted (e.g. in religious and family systems), the new in other institutions, such as in technology, is not only considered acceptable, but is in fact eagerly sought. The former may be termed predominantly 'sacred' institutions, and the latter may be termed predominantly 'secular' contexts. 'Sacred' here does not only refer to the 'holy'. It includes the holy, but is used in a more general sense, meaning general reluctance to change. 'Secular' is also used in a somewhat distinctive sense: It refers to greater or smaller acceptance of change.⁴

In general, it is reasonable to expect that, other things being equal, events will more readily be unanticipated in our limited sense in predominantly sacred institutions than in predominantly secular contexts. Change and new events are, in the latter case, accepted and often eagerly sought as ends, and such contexts do not easily foster unanticipated events.

In connection with the importance of the institutional context, the place of *scientific activity* becomes especially interesting. Seeking the new is certainly part and parcel of scientific activity. In fact, the new is anticipated: scientific hypotheses may be considered formalized anticipations about the new. In a way science is the most highly 'secularized' institution in our society (in the special sense referred to above). In turn one may consider this secularity as somehow related to the ofte 'cool and imperturbable wonder' of the scientist; a kind of curiosity which is not closely related to 'astonishment'. But in spite of the scientist's being socialized in secularity, and as an apparent contradiction to his frequently calm wonder about nature and the future, genuine astonishment will very frequently be the reaction when hypotheses are not verified, and perhaps especially when null-hypotheses cannot be rejected. In other words, the state and signs of astonishment may be very strong indeed when certain things occur in this secular context. The point here is that though part of the code of the scientist certainly is acceptance of and seeking for the new (secularity), another part of his code consists of clear specification of *exactly* what new is anticipated, and no actual acceptance of the occurrence of other things than those specified in hypotheses *on face value*. Clear specification of exactly what is to be expected implies that many things are very strongly anticipated. This, in turn, leads to reluctance to accept, on face value, alternatives to those events specified in hypotheses. The unanticipated events must first be carefully explained. In this round-about way events may be even *strongly* unanticipated (and consequently astonishing) in this apparently secular context. To put the matter differently, science actually has some rather strongly 'sacred' traits.⁵

So much about the relationship between the institutional context and the unanticipated (and astonishment). Given the occurrence of something unanticipated (unforeseen and mysterious), what other factors may, together with the unanticipated, contribute to the occurrence or strength of astonishment? There are no doubt many. Let us briefly mention a rather important one as an example:

Closely related to earlier statements about the mysterious, the occurrence or at least the strength of the state and signs of astonishment may in part depend on how strongly held or well founded a given predictive expectation about the future is, or, in the case of the second type of the unforeseen, how 'blue the sky' actually is. This means that *general knowledge* of the context or field within which the unanticipated event takes place may be important. We do not consider it fruitful to spell out

all the hypotheses that are possible in this connection. Suffice it to say that a great amount of contextual, general knowledge may, under some conditions, be helpful in producing the state and signs of astonishment, and under other conditions it may contribute to reduce the strength or probability of the occurrence of the reaction.⁶

These comments by no means constitute an exhaustive analysis of the conditions under which the reaction of astonishment occurs or of the factors conditioning the amount of astonishment. However, enough has perhaps been said to indicate the great complexity of such an ordinary type of phenomenon.

It may further be mentioned that though astonishment may be associated with such factors as those referred to above, other reactions may also follow from combinations of such conditions, such as feelings of the comical, feelings of having been caught redhanded, fear, rage, hatred, and the like. Such reactions will often in part be consequences of unanticipated events, and they may of course occur together with astonishment.

Now, how can astonishment be related to general sociological conceptions of social action? It may be viewed as a *means*, as an *end*, and as a *condition* of action.

III. Astonishment and the elements of action

Actions may conveniently be analysed in terms of four basic elements: actor, means, ends, and conditions.⁷ For the purposes of the present exposition, 'actor' will simply be taken to refer to the point of reference of the analysis. As indicated already, 'actor' refers to the person who becomes astonished.

Means, ends, and conditions are actually quite commonplace concepts in sociological analysis. However, it is very important to note that we shall, as may be deduced from earlier statements, concentrate on these elements of action from the subjective point of view of the actor, and not externally or from the point of view of the observer. We will treat astonishment as a *conscious* means and as a *conscious* end, and not, for example, as an unconscious, hidden motive of action. Furthermore, we shall treat astonishment as a condition only in so far as it is perceived as a condition by the actor in question. Therefore we shall deal with unintended functions of being astonished only in so far as the functions are perceived or recognized by the actor.⁸

1. Astonishment as a means in action

"I am surprised at you." "I hadn't expected that from you." The raised eyebrow is a well-known means of controlling the allegedly unanticipated actions of others. Also, stronger degree of overt manifestation of being astonished may be used as such controlling means. In other words, astonishment may be utilized consciously in order to reach a desirable future state of affairs.

However, further differentiation of types of astonishment used as a means is necessary. A distinction must be made between the subjectively genuine state of astonishment, the signs of which are almost immediately and purposely employed by the actor as a means of control, and signs of astonishment consciously mustered for the occasion. In the latter case, the astonishment is subjectively false.

In the former case, an event may actually be unanticipated by the actor, overt signs of astonishment occur as a consequence of a genuine inner state, and these essentially unwilling signs are immediately 'converted' and *purposefully* manipulated for control. The essential point is that the genuine signs of astonishment, when perceived by others, bring the unanticipated character of the event into the open, and this openness is more or less purposely combined with moral evaluation of the event. This is a form of social control that has a rather important character. Ideal-typically it does not include any other concrete action than simply stating clearly (though initially unwilling) that the event constitutes an exception to anticipatory expectations *and* that it is also deviance from normative expectations. The success of such a control will obviously vary with the larger context of action, such as the status of the person being astonished and the extent to which the overt signs of astonishment may, if necessary, be followed up by other, more powerful means. And often it is used, for example, in order to win time needed to explore the possibilities of other means. Nevertheless, it must be emphasized that astonishment here exemplifies one special category of social control, in so far as the immediate controlling mechanism simply consists in *stating* to others that an event is unanticipated and (un)desirable, *and nothing more*. Many means of social control imply more 'direct' sanctioning action than just stating this. However, it should of course be remembered that astonishment as such a means of control is, in our society, perhaps mainly effective against less serious deviance.

As mentioned above, a show of being astonished may be consciously and (from a subjective view-point) 'falsely' mustered for the occasion. In this case, we do not deal with a genuine state of astonishment and

initially unintended signs of the state, but only with the manifestation of the usual overt signs without the corresponding genuine 'inner' state. Such pretended or intended astonishment may be used as a means of control when the event in question is in fact not fully unanticipated (either because it is foreseen or not mysterious or both). The event is treated *as if* unanticipated.

Such conscious mustering of the signs of astonishment *may* have the same implications as when the signs are based on a genuine state. This may be true even when the lack of genuineness is grasped by *alter*, and even when this understanding is in turn understood by *ego*. The control may then still be effective.

Now, a distinctive trait of this type of control appears when we shift focus from deviating *alters* to deviating *ego*. Above, we have treated the signs of astonishment, whether based on a genuine state or not, as a means of controlling others in the environment. However, these signs may also be used consciously to hinder *oneself* from deviating from the normatively acceptable or obligatory. But it seems to be the nature of the case that as such the signs can only be used if they are based on no genuine state of astonishment. They must be pretended or 'false'. To take two examples: A person pretends to be astonished when listening to a friend's dull narrative over the telephone; a person pretends to be astonished when hearing the solution to a puzzle that he has not understood at all. In both cases, the pretended signs of being astonished, which for example may be used in order to win time, are consciously employed by the person in order to stay within the limits of the normatively acceptable or even obligatory. This is so because in these cases perception of the unanticipated belongs to the normatively correct. The usual signs of astonishment cover up one's own possibilities of deviance; they help the actor keep certain parts of his self *secret*.⁹ The effectiveness of this control of one's own deviance hinges of course on the condition that the pretended character of the signs is not understood by relevant *alters*.

2. Astonishment as an end in action¹⁰

Astonishment may be sought as a desirable future state of affairs; as an end in action.

Several sociologists have pointed out the importance as ends or 'motives', etc. of phenomena that are fairly close to astonishment. In this connection, one of William I. Thomas' famous 'four wishes', 'the

desire for new experience', should have honourable mention as a classical example.¹¹

Furthermore, the value of 'the thrilling' stressed by Howard Becker as the pole of one of his continua of degree of acceptance of change¹² is highly relevant. By stress on the value of the thrilling, Becker refers to quite complete acceptance of the new.

However, the precise relationship between astonishment, new experience, and the thrilling as conscious ends in action is quite complex and needs some clarification.

It was mentioned earlier that when new events are accepted and perhaps even sought as ends (secularity), it may be somewhat difficult for events to be unanticipated and thereby astonishing. Yet, the *particular* new experience may in many contexts be unanticipated when the new in general is sought as an end. The *particular* new event may on this basis be quite astonishing. This seems, for example, to be one part of the enchantment of games of chance. Astonishment will often be one realistic part of the goal when such games are played: Even though new experience is certainly sought, the fact that *I* win a given game of chance is unanticipated.¹³ I had definitely not expected to win, and this expectation is contradicted. Furthermore, the fact that *I* win is often subjectively 'mysterious', even though it is entirely reasonable in terms of objective characteristics of games of chance. The process outlined here seems to be the essence of some types of seeking new experience.

Note that the element of the unforeseen enters here in the sense of contradiction of prior expectations and that we are dealing with astonishment as a conscious end. We shall later come back to the relationship between 'bolts from the blue' (no prior expectations at all) and astonishment as an end.

From the earlier analysis of scientific activity in relation to the unanticipated, it will be seen that scientific hunting for the new is actually of the type outlined here. Science includes precise anticipation about the particular new event, and events may then certainly be unanticipated and thereby astonishing. Yet it must be remembered that there is at least one fundamental difference between these two cases of seeking the new: In relation to games of chance and similar activities, astonishment is thought of as an end, whereas in relation to science, it is considered more of a nuisance than anything else, and certainly not as a desirable future state of affairs.¹⁴

Now, avoidance of correct knowledge of what is actually going to happen can often not be maintained in the long run when the new in

general is accepted and sought as an end, and if it is not maintained, astonishment may be defeated as an end. Knowledge, though a general value, is here dysfunctional; ignorance is functional. In modern Western cultures such defeats seem to be frequent, pointing to a 'disenchantment' in our secular society.

We will come back to a common attempt at a solution of such disenchantments in a moment. But we must first briefly comment on the relationship between astonishment and the thrilling as ends: The state of astonishment may be said to be an important aspect of the thrilling; it may in fact in many contexts be the most significant part of the goal. Yet the thrilling often has other important components, such as the comical. It is recognized that such things as the astonishing, the comical, etc. are often fused from the point of view of the actor, constituting generalized thrill as opposed to general boredom. However, for purposes of analysis, we may try to single out 'the dimension' of astonishment, though many of the comments below are as relevant to the generalized thrill as to the more specific astonishment, and though the separation of astonishment from other aspects of the thrilling can admittedly not be complete.

We may continue with a couple of examples, thus trying to approach a usual attempt at a solution of the above-mentioned disenchantment.

The 'amusement park' may be viewed as implying institutionalized acceptance of new experience and the thrilling as conscious ends in action. Two of the most astonishing features of the amusement park are the *magician* and rooms with *distorting mirrors*. These contrivances are designed to astonish, and to some extent they do in fact astonish. And here we are up against a rather important phenomenon. Unanticipated events will often be astonishing. And we have indicated how, when new experience is a goal, an element of the unanticipated with regard to the *particular* new experience may still lead to astonishment. But a great part of the events in rooms with distorting mirrors and in the performances of the magician is not in the long run unanticipated in the strict sense, and yet they may — at least to some extent — *continue to astonish*¹⁵; in fact, the magician in our Western culture may be viewed as a professional 'astonisher'. We have here a particularly important example of the reaction of astonishment to seemingly recurrent and strictly speaking anticipated events.

Now, a person may be astonished here simply because the events pose the problem of the technically mysterious. Though clearly extremely important, this is hardly all that matters, because such inventions as

distorting mirrors may continue, among other things, to astonish even when explained. While recognizing the importance of the mysterious alone in relation to astonishment, it will therefore be worth while to look for other contributing factors.

True, we may anticipate, in the sense of the word used here, that the magician will make the coins disappear or get his rabbits out of the hat¹⁶. And we may anticipate that the distorting mirrors will reflect comical and uncomplimentary pictures of ourselves. However, in all of this it is very important to keep in mind the actor's frame of reference: These events are still in one somewhat attenuated sense 'unanticipated', *in so far as they are separated from and compared with the routine of everyday life*. Rabbits are usually not pulled out from hats, and in a sense, from the point of view of the every-day, they are 'mysterious' and 'unforeseen'. If the activities of the magician or the events in the rooms with distorting mirrors were viewed in any or no relation to the every-day, many of them would quickly cease to astonish. The point is that they are viewed in one very specific relation to the every-day. Though these events are often very repetitive and much alike, and though they may be explained in a strict sense, they constitute 'unanticipated' events in the sense of being viewed as outside exceptions to the routine of the everyday. Subjectivistically, they are at least close to the unanticipated. Here lies a kind of solution to the disenchantment discussed above. The state of being astonished is at least to some extent realistic as an end and is in fact to some extent attained as such as long as there is a separation and comparison between the routine of the 'week-day' and the 'day out'. Such clear separation between the necessary routine on the one hand and the entertaining on the other is possibly limited to certain cultures, including our own. In cultures where separation between these spheres of life is less clear and basic, astonishment (and the thrilling and exciting in general) possibly takes on a rather different character.

However, it is a well known fact that lessening of the effect and subsequent boredom will often take place. Boredom, and particularly one aspect of it, namely lack of fulfilment of the goal of astonishment, seems often to ensue when it becomes impossible to view the events as 'unanticipated' even in the attenuated sense discussed above. In other words, the 'solution' is very often not permanent. Not that rabbits are constantly pulled out of hats. But magicians are usual and thereby 'anticipated' in a series of places and contexts, and so are rooms with distorting mirrors, and by this token they more or less enter the routine of the every-day.

More precisely, there is no longer a clear separation and comparison between the every-day and the exciting free-day or day out. The two are fused subjectivistically, and what was formerly in a sense 'unanticipated' is now anticipated. Astonishment is then often not realistic as an end.

Suffice it to conclude here that when this aspect of boredom follows, new, allegedly more astonishing events may be sought, and a process may be started in the direction of more or less supreme acceptance of anything that may possibly give a 'kick'. This may perhaps be called normless secular search for the astonishing, or more generally for the thrilling.¹⁷

So far we have discussed rather specific examples of activities where astonishment may be viewed as a (more or less realistic) end, and we have tried to point out the importance of separating the every-day from the day out when searching for astonishment. Subjectivistically, the event in question may then become 'unanticipated'. However, it seems that the unanticipated is also of more general significance.¹⁸

At least a certain amount of 'unanticipatedness' and subsequent astonishment may be of considerable importance under many conditions when it comes to the long-term maintenance of such a social relationship as love, and possibly also of friendship. A kind of astonishment may here be part of a more general end. Lack of future possibility of being astonished at others seems to be an important part of the essence of being bored with others. Furthermore, the long term success of such social relationships seems in part to hinge on a proper balance between the astonishing and the not-astonishing.

To mention only one other example, such a 'proper balance' seems to be part of the enchantment of music, and probably other forms of art as well. Much of the delight of music stems from a balance between the well-known *and* the new, — from recognizing *and yet* being startled, — from anticipating the future (in terms of form, harmony, melody) *and* being in a sense 'astonished'. In fact, the feelings of anticipating and astonishment occur practically at the same time. The separation here between the anticipated and the unanticipated does not occur along a time-dimension; a given event is felt as anticipated and unanticipated *at the same time*.

From this point of view, such varied social forms as interaction between magician and audience, love, friendship, and the relation between music and the listener basically imply the same end: a kind of astonishment based on the unanticipated. Furthermore, this end must probably be reached from time to time if the structures in question are to be maintained over any length of time.

But note the condition that was mentioned earlier of *proper balance* between the astonishing and the not astonishing. Lack of astonishment and complete predictability may be devastating and lead to the 'meaningless', but so may too much astonishment, in love as well as music.

Now, let us by way of concluding this section try to circumscribe more closely the *type* of unanticipated that may realistically be connected with astonishment *as an end*.

New experience and some aspects of the thrilling may realistically be sought as ends when the event in question is mysterious, and unforeseen in the sense of having *no prior predictive expectations connected with it at all*. But then *astonishment cannot be a realistic part of the end*. In other words, astonishment cannot be a realistic end when the 'field' is completely open or new, or when there is conscious or unconscious suspension of all prior predictive expectations.

Extreme cases of lack of predictive expectations will probably often be associated with more or less complete lack of normative expectations. Hence, we may call this situation normless or anomic.

To be more precise, it is here maintained that the type of the unanticipated where an event is unforeseen in the sense of being 'a bolt from the blue' is not relevant for astonishment *as an end*. The astonishment that may follow from such an unanticipated event cannot be an *end* of action, in the sense of being a conscious goal. Phenomenologically, it may take on a form or lead to a feeling that is not terribly far from an end, such as when 'a bolt from the blue' is felt as a 'pleasant surprise'. But though such passive acceptance of the pleasant astonishment is important, it does not constitute a conscious goal.

Astonishment, then, can be perceived as a realistic end and can be attained as an end neither when the formerly unanticipated has become anticipated (boredom) nor when everything belongs to the second type of the unforeseen (anomie).

3. Astonishment and embarrassment

To repeat: some, though by no means all, of the above-mentioned constellations of conditions may, produce *surprise* in the sense of being caught unawares. In the present section, we will treat some recognized functions of what we call astonishment that certainly may be functions of this sense of feeling surprised. Yet, we will limit ourselves to these functions as consequences of 'astonishment'.

By a 'condition in action' is meant a factor in the environment that the

actor cannot manipulate in order to reach his ends but that still is perceived as being unintentionally eufunctional or dysfunctional for the attainment of ends.¹⁹ As a condition, astonishment intervenes between the occurrence of an unanticipated event and what the actor considers as his end or ends. Astonishment may on the one hand be viewed as a problem-solving or eufunctional condition for the attainment of ends. For example, the overt signs of astonishment perceived by others may bring the existence of a problem into the open, thus making possible concerted action on the problem. Furthermore, the state of astonishment may, in science as well as in other contexts, be perceived as helpful in sensitizing the actor toward new courses of action, new discoveries, etc.

However, astonishment may on the other hand be a problem-creating or dysfunctional condition. Of the several possible dysfunctions of astonishment as a condition, we may concentrate on an important recognized²⁰ (though in the nature of the case unintended) one: Astonishment perceived by the actor as a direct or indirect source of *embarrassment*. The signs of astonishment may aggravate the problems of reaching a given end when the reaction is viewed as a source of embarrassment.

The signs may transmit the occurrence of something unanticipated to others. By such tokens, others will easily believe that the actor is not quite able to cope with the unanticipated event. When thus unintended, showing signs of astonishment easily 'implies' lack of control of the situation. Thus, astonishment becomes a muddy spot on the actor's presentation of self.

The essential point here may be that the occurrence of something unanticipated will easily be considered something private; — a part of one's activities that should not be presented to others. The situation is in fact considered 'under control' as long as the unanticipated character of the event in question is kept private. In other words, the important matter becomes one of 'saving face'. Signs of astonishment may hinder such face-saving in this context, and they break the secret of the unanticipated. On the basis of this, astonishment becomes embarrassing, which in turn further hinders effective attainment of ends.

This, it may be maintained, is only one aspect of the general problem of breaking secrets. The occurrence of a fully anticipated event may, when actually part of a secret, be the source of embarrassment when perceived by others, even though there is no intervening process of astonishment. Yet (and this is the main point) the signs of astonishment may break the secret unusually crudely, and without giving much chance to cover up. When a person shows signs of astonishment, the secret is easily

irretrievably broken. When secrets are broken in other ways, they may often be *more or less* broken, but when the signs of astonishment break the secret, the breaking will easily be more complete.

This is obviously related to the hypothesis that in our culture there is a sharp separation between the private and the public; between 'back-stage' and 'front region'.²¹ Furthermore, it is based on the assumption that the unanticipated often belongs to the private — that it often will be thought of as a secret — and in turn that lack of immediate 'control of the situation' is considered part of the private sphere.

It may be repeated that the signs of astonishment are here viewed as having unintended functions. Furthermore, we base our argument on the notion that the signs of astonishment are themselves unintended; they are not consciously mustered for the occasion. We have earlier seen that the signs of astonishment may have intended functions opposite of the ones outlined here, for example in so far as the signs (when intended in and by themselves) may cover up the actor's deviations and help the individual *keep* secrets. However, it is clear that the cases are based on other differences in conditions as well.

V. Concluding comments

The present paper has dealt with some selected aspects of the subjective state and overt signs of what may be called 'astonishment', a very common phenomenon that all human beings encounter. The treatment, which does not pretend to be exhaustive, is most directly relevant to face-to-face relations, though in principle it is also relevant to other types of situations.

Astonishment has been treated in relation to the unanticipated (in the sense of not foreseen and mysterious), and as a means, end, and condition in action. The analysis is limited to Western culture.

As treated here, signs of astonishment, when used as a means of controlling *others*, imply that the unanticipated event (or the event that is pretended to be unanticipated) constitutes unwanted *deviance* from the normative, while when the state of astonishment is an end, the event is a positively evaluated *innovation*. In both cases, a decision is made with regard to the important question of whether the unanticipated is to be interpreted as a negative disturbance of equilibrium or as a positive new addition to and change of the system in question.²²

NOTES

- ¹ See Neal Gross, Ward S. Mason, and Alexander W. McEachern, *Explorations in Role Analysis*, New York: John Wiley & Sons, Inc., 1958, esp. pp. 58-60. See also Herman Tennessen, "What Should We Say", *Inquiry*, Vol. 2, No. 4, 1959, pp. 265 ff.
- ² It is freely admitted that there are important exceptions to this rule. The following are relevant here: Robert K. Merton, "The Unanticipated Consequences of Purposive Social Action", *American Sociological Review*, Vol. 1, No. 6, December 1936, pp. 894-904; Wilbert E. Moore and Melvin M. Tumin, "Some Social Functions of Ignorance", *American Sociological Review*, Vol. 14, No. 6, December 1949, pp. 787-95; Bernard Barber and Renée C. Fox, "The Case of the Floppy-eared Rabbits: An Instance of Serendipity Gained and Serendipity Lost", *American Journal of Sociology*, Vol. LXIV, No. 2, Sept. 1958, pp. 128-36; Vilhelm Aubert, "Chance in Social Affairs", *Inquiry*, Vol. 2, No. 1, Spring 1959, pp. 1-24. It should be noted that the interest in the unforeseen and unanticipated is not a completely new development in sociology. To mention a couple of examples, Max Weber dealt, however briefly, with the phenomenon of chance in relation to succession of leadership, and aspects of the 'unknown' constituted a highly significant component in the sociological theories of William I. Thomas (We will come back to Thomas' contribution later). These works are also exceptions to our rule. Furthermore, there are no doubt quite a few non-sociological works that deal with phenomena that are unanticipated or based on something unanticipated.
- ³ In apparent contradiction to the statement that the mysterious is a necessary condition of astonishment, some types of reactions come quite close to astonishment even though the trigger event is neither unforeseen nor mysterious. For example: "Why does this happen to *me*? I can't understand it". *One part* of the feeling implied here may be a kind of vague 'hopeless astonishment', even if the immediate event is foreseen and explained. However, it must be admitted that though the immediate event may be explained, the larger context will usually, rationally or irrationally, be 'inexplicable'. Therefore, even such events seem to have the mysterious as a condition.
- ⁴ Here Howard Becker's terminology is followed fairly closely. See for example Howard Becker, *Through Values to Social Interpretation*, Durham: Duke University Press, 1950, chap. V.
- ⁵ We will later come back to the importance of the new.
- ⁶ The last paragraph is replete with problems, aside from the thorny one of specifying the conditions under which general knowledge of the context furthers and hinders astonishment. For example, the distinction between general knowledge of the context and specific anticipations is a vague one.
- ⁷ Talcott Parsons, *The Structure of Social Action*, Glencoe: The Free Press, 1949, pp. 43 ff.; Kingsley Davis, *Human Society*, New York: The MacMillan Company, 1949, chap. V.
- ⁸ Recognized and unintended phenomena constitute an empirically important category of consequences. Such consequences are actually a combination of so-called 'manifest' and 'latent' functions. See Robert K. Merton, *Social Theory and Social Structure*, Glencoe: The Free Press, 1957, chap. I.
- ⁹ We will later come back to the fact that the signs of astonishment may have exactly the opposite unintended function when they are based on a genuine state.

- ¹⁰ It should be noted that in the present section we shall concentrate more on the 'inner state' of astonishment than on overt manifestations of it. In popular usage, astonishment sometimes has a negative connotation. However, no such negative connotation is necessarily implied here.
- ¹¹ 'The four wishes' occur in a series of places in Thomas' writings. His last formulation of them appeared in *The Unadjusted Girl*, Boston: Little, Brown, and Company, 1925.
- ¹² See for example Howard Becker, *Man in Reciprocity*, New York: Frederick A. Praeger, 1956, esp. pp. 174-6.
- ¹³ See Aubert, *op. cit.*, for an extensive treatment of the phenomenon of chance. Incidentally, it should be stressed that astonishment is seldom if ever the whole end in games of chance.
- ¹⁴ We are here touching on the matter of astonishment as a condition in action, which will be dealt with in more detail below.
- ¹⁵ As indicated earlier, there are of course also other qualities of the thrill present, such as other reactions than astonishment to the often impressive technical character of the performance of the magician.
- ¹⁶ In fact, we would be highly astonished indeed if he did not manage to. This is in fact employed as a trick by some magicians.
- ¹⁷ See Becker, *Man in Reciprocity*, *op. cit.*, chap. XII, for a somewhat similar treatment of this last point.
- ¹⁸ I am here indebted to Dr. Vilhelm Aubert for helpful discussion. In the paragraphs that follow, we actually imply that an end may at the same time be a means, or at least have positive functions with regard to reaching another, more distant end. Furthermore, a means may of course also be viewed as a short-term end. *Large parts* of this paper may in fact be considered in terms of such a means-end 'chain', though we have not treated it explicitly in the text.
- ¹⁹ Though the present conceptual framework relies much on Kingsley Davis, *op. cit.*, we do depart somewhat from Davis' usage of the concepts involved. In particular, we depart from his usage of the concept of 'condition'.
- ²⁰ Thus keeping in line with our subjectivistic point of departure. Incidentally, the state and signs of astonishment many of course be irrelevant elements in action. We will not treat this possibility here.
- ²¹ Erving Goffman, *The Presentation of Self in Everyday Life*, Edinburgh, 1956.
- ²² For a treatment of deviance and innovation in relation to equilibrium, see Theodore M. Mills, "Equilibrium and the Processes of Deviance and Control", *American Sociological Review*, Vol. 24, No. 5, October 1959, pp. 671-9, esp. pp. 673-4.

ARISTOTLE'S DIFFERENT POSSIBILITIES

by

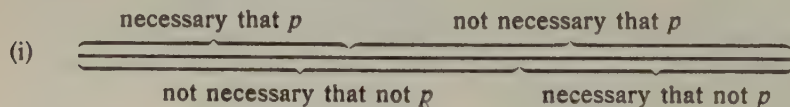
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1. *The interrelations of modal notions in Aristotle.* The results of our examination of the varieties of ambiguity in Aristotle (see *Inquiry* vol. 2 (1959), pp. 137—151) can be used to analyze his notion of possibility. This notion is closely connected with the other modal notions, notably with those of necessity and impossibility. Since these notions are somewhat more perspicuous than that of possibility, it is advisable to start from them.

Aristotle knew that the contradictory (negation) of 'it is necessary that p ' is not 'it is necessary that not p ' but rather 'it is not necessary that p ' (*De Int.* 12, 22^a4 ff.). The last two phrases are not contradictories, either, for they can very well be true together (*De Int.* 13, 22^b1 ff.), the latter being wider in application than the former. By parity of form, 'it is not necessary that not p ' is wider in application than 'it is necessary that p '.

These relations are conveniently summed up in the following diagram (which is not used by the Stagirite):



According to Aristotle, 'impossible' behaves like 'necessary' (*De Int.* 12, 22^a7 ff.). We can therefore illustrate it by means of a diagram similar to (i). In fact, the diagram will be virtually the same as the one for 'necessary', for "the proposition 'it is impossible' is equivalent, when used with a contrary subject, to the proposition 'it is necessary'." (See *De Int.* 13, 22^b5.) In other words, 'impossible that p ' is equivalent to 'necessary that not p ', 'impossible that not p ' equivalent to 'necessary that p ', etc. We can therefore complete the diagram (i) as follows:

	necessary that $p =$ impossible that not p	not necessary that $p =$ not impossible that not p
(ii)	<hr/> <hr/>	
	not necessary that not $p =$ not impossible that p	necessary that not $p =$ impossible that p

2. *The two notions of possibility.* The problem is to fit the notion of possibility into the schema (ii). In this respect, Aristotle was led by two incompatible impulses. On one hand, he was naturally tempted to say that 'possible' and 'impossible' are contradictories: something is possible if and only if it is not impossible. (See e.g. *De Int.* 13, 22^a16—18, 32—38.) Under this view, we get the following diagram:

	not possible that not $p =$ necessary that p	possible that not $p =$ not impossible that not p
(iii)	<hr/> <hr/>	
	possible that $p =$ not impossible that p	not possible that $p =$ impossible that p

But this temptation is not the only one. In ordinary discourse, saying that something is possible often serves to indicate that it is *not* necessary. Aristotle catches this implication. For him, "if a thing may be, it may also not be" (*De Int.* 13, 22^b20; see also 22^a14 ff.). Essentially the same point is elaborated in the *Topica* II, 7, 112^b1 ff. There Aristotle says that "if a necessary event has been asserted to occur usually, clearly the speaker has denied an attribute to be universal which is universal and so has made a mistake."

Under this view, our diagram will have this look:

	necessary that $p =$ impossible that not p	possible that $p =$ possible that not p	impossible that $p =$ necessary that not p
(iv)	<hr/> <hr/>		
	not impossible that p		

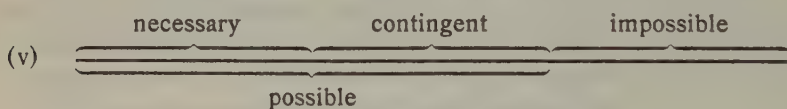
It is seen that (iii) and (iv) differ in that in (iii) the range of possibility comprises everything that is necessary, while in (iv) possibility and necessity are incompatible.

It appears from *De Interpretatione* that Aristotle did not immediately see that the assumptions underlying (iii) and (iv) are incompatible. Not surprisingly, he ran into difficulties which he discusses in a not entirely clear way in *De Int.* 13, 22^b11—23^a7 (although I suspect that the confusion of the usual translations of this passage is not altogether Aristotle's fault). He perceives clearly enough that the gist of the difficulty lies in the relation of

possibility to necessity (*De Int.* 13, 22^b29 ff.). And at the end he is led to distinguish two senses of 'possible' one of which satisfies (iii) and the other (iv). (*De Int.* 13, 23^a7—27.) However, Aristotle does not make any terminological distinction between the two. Insofar as the distinction is vital, I shall call the notion of possibility which satisfies (iii) 'possibility proper' and the notion which satisfies (iv) 'contingency'.

3. *Homonymy v. multiplicity of applications.* Now we can see why the distinction between a diversity of applications and homonymy is vital for this essay. We have found a clear-cut case of homonymy: the notions of contingency and of possibility proper have different logical properties. They cannot be covered by a single term 'possibility' except by keeping in mind that this word has different meanings on different occasions. Their relation is therefore one of homonymy (*cf.* section 10 of the first paper).

But in addition to this duality of 'contingency' and 'possibility proper', there is a different kind of distinction. One of these two logically different notions, *viz.* possibility proper, covers *two kinds of cases*. When one says that *p* is possible (in the sense of possibility proper), one sometimes could also say that *p* is contingent and sometimes that *p* is necessary. This does not mean, of course, that the term 'possible' is ambiguous; it merely means that its field of application falls into two parts. It was for Aristotle therefore a typical case of multiplicity of applications as distinguished from homonymy (*cf.* section 10 of the first paper). The following diagram makes the situation clear:



The distinction between the different applications of 'possibility proper' loomed large for Aristotle because he tended to emphasize the distinction between necessity and contingency. Thus Aristotle argues in *Met.* X, 10 that the perishable and the imperishable are different in kind (*σῆδος* and *γένος*). In an earlier paper ('Necessity, Universality, and Time in Aristotle', *Ajatus* vol. 20 (1957), pp. 65—90), I have argued that the distinction between contingency and necessity is for Aristotle equivalent to that between what is perishable and imperishable. The field of application of 'possibility proper' therefore falls into two parts which are different in kind. We have already seen in sections 10 and 12 of the first paper that Aristotle viewed situations of this kind with suspicion, although he grudgingly admitted that no logical harm need result. This is probably one of the reasons why Aristotle in *Analytica Priora* preferred the notion of contingency to that of possibility proper.

4. *Aristotle's definition of contingency.* According to the results of our

examination of the ambiguities of Aristotelian ambiguity, we may expect that the Stagirite usually refers to the distinction between possibility proper and contingency by means of ὁμωνυμία and that he always refers to the distinction between the different cases of possibility proper (*i. e.* between necessity and contingency) by means of some other locution, *e. g.* διχῶς λέγεται, κατὰ δύο τρόπους λέγεται or πολλαχῶς λέγεται. An examination of the text will bear out this expectation as far as the first, the second, and the fourth expressions are concerned. Similarly, the third locution is, we shall find, used by Aristotle in distinguishing two kinds of cases of contingency (see section 6 below).

In Aristotle's discussion of the notion of possibility, the key passage is in *An. Pr.* I, 13, 32^a18—21. It is referred to by Aristotle repeatedly as the definition of possibility (*e. g.* *An. Pr.* I, 14, 33^b24; 15, 33^b28; 15, 34^b28; 17, 37^a27). The 'definition' is clear enough (I shall not discuss here why Aristotle thinks of it as a definition):

I use the terms 'possibly' and 'the possible' of that which is not necessary but, being assumed, results in nothing impossible.

This is clearly the notion I have called contingency. However, it is not the only variant of possibility, for Aristotle continues:

τὸ γὰρ ἀναγκαῖον ὁμωνύμως ἐνδέχεσθαι λέγομεν

That is, to say of the necessary that it is possible is to use the term 'possible' homonymously. This explanation obviously serves to motivate the qualification "which is not necessary" in Aristotle's definition. The use of the word ὁμωνύμως shows that he knows that he is making a choice between two incompatible meanings of ἐνδέχεσθαι (to be possible). The second meaning, under which even necessary things are called possible, is the notion of possibility which satisfies (iii) and which I have called possibility proper.

We have thus reached two important conclusions: (a) the main notion of possibility employed by Aristotle in *An. Pr.* is what I have called contingency; (b) Aristotle is aware of the existence of the other notion (possibility proper) which is different from contingency to the degree that the same term can be applied to them only homonymously.

These results are confirmed by other passages. A glance at (iv) shows that contingency is symmetrical with respect to negation: *p* is contingent if and only if not-*p* is also contingent. They are therefore convertible to each other. Aristotle makes the same observation and applies it to syllogistic premises in *An. Pr.* I, 13, 32^a30 ff. This shows that his 'possibility as defined'

agrees with my 'contingency'. Essentially the same point is made in *An. Pr. I*, 17, 37^a22 ff.

Aristotle's awareness of the ambiguity of possibility is also demonstrated by the development of his syllogistic. He frequently points out that the conclusion of a certain syllogism is valid only if one does not understand possibility in the sense defined (*i.e.* in the sense of contingency) but in a sense in which it is the contradictory of impossibility (*e.g.* *An. Pr. I*, 15, 33^b30—33; 34^b27—32; 16, 35^b33; 17, 36^b33; 20, 39^a12).

5. *An analysis of An. Pr. I*, 13, 32^a21—28. The fact that Aristotle was aware of the different logical properties of contingency as distinguished from possibility proper seems to me to be in agreement with what Aristotle writes immediately after the passages I have quoted (*An. Pr. I*, 13, 32^a21—28). This passage has been censured by the recent commentators in spite of the fact that it occurs in all the MSS and is recognized both by Alexander and by Philoponus (W. D. Ross, *Aristotle's Prior and Posterior Analytics*, Oxford, 1949, p. 327). However, it seems to me that the passage can be understood as it stands by making due allowance for Aristotle's conspicuous conciseness. I shall offer a paraphrase of the passage, enclosing explanatory additions as well as my own comments in brackets. The superscripts refer to further comments.

Aristotle has just explained his sense of ἐνδεχόμενον (possible) and distinguished it from the homonymous notion of possibility proper. He goes on:

That this [= Aristotle's definition] is the meaning of 'possible' is obvious from the opposing affirmations and denials.¹ For [in the other sense of 'possible'] 'it is not possible to apply', 'it is impossible to apply' and 'it is necessary not to apply' are either the same or imply each other.² Consequently their contradictories³ 'it is possible to apply', 'it is not impossible to apply' and 'it is not necessary not to apply' are the same or imply each other. For either the affirmation or the negation always applies. [This is not correct, however, for we mean by possibility something more than the absence of impossibility.⁴] That which is necessary will therefore not be possible, and that which is not necessary [nor impossible⁵] will be possible.

Further comments: (1) This elliptic sentence poses two questions:

- (a) What are these affirmations and denials affirmations and denials of?
- (b) What kind of opposition is Aristotle here referring to?

As regards (a), the sequel shows that Aristotle is not dealing with affirmations and denials of possibility in the sense (of contingency) just defined. It turns out (*cf.* (2) below) that the affirmations and denials pertain to the other sense of possibility (possibility proper). Since Aristotle is obviously

trying to justify his own definition, it may be concluded that he is here starting a *reductio ad absurdum* argument.

As regards (b), a comparison with the occurrences of ἀντικείμενος later in the passage (cf. (3)) suggests that this word — which is Aristotle's vaguest and most general term for opposition of any kind — here refers to contradictory opposition. The alternative would be to understand the sentence as referring to the opposition between the two kinds of possibility; this would suit my interpretation quite as well as the other reading.

(2) This is exactly what we get by accepting the other sense of ἐνδεχόμενον, *i.e.* by not excluding necessity from the range of possibility: 'not possible that *p*' will be equivalent to 'impossible that *p*' which is (cf. diagram (ii)) tantamount to 'necessarily not *p*'.

(3) The following sentence shows that these contradictories are the ἀντικείμενα referred to here.

(4) It was already pointed out above (in section 2) that Aristotle took this view. See especially the reference to the *Topica*, *loc. cit.* It appears from the φανερόν in 32^a20 that Aristotle thought of this point as being perfectly obvious; so obvious, indeed, that he neglected to make it explicit here.

(5) The second part of the last sentence seems strange. The addition I have indicated is a most tempting way of making the passage correct. It is very likely, however, that the passage is Aristotle's as it stands. He knew that his notion of possibility (*i.e.* contingency) is symmetrical with respect to negation in the sense which best appears from diagram (iv). He may have thought that this symmetry justifies the transition from 'what is necessary is not possible' to 'what is not necessary is possible'. This leads to a reading of 'not necessary' as an elliptic form of 'not necessary either way', *i.e.* 'neither necessary nor impossible'. In the sequel, we shall find more indications that this was Aristotle's reading; see section 7 *infra*.

Here I shall only point out that my interpretation is supported by what we find in *De Interpretatione*. If it is true that 'not necessary' sometimes does duty for 'neither necessary nor impossible', it may be expected that 'not necessarily not', *i.e.* 'not impossible', will sometimes mean 'neither impossible nor necessary'. When this is so, 'not impossible' will entail (in fact, it will be equivalent to) 'not necessary'. And this is exactly what we find in *De Int.* 13, 22^b14—16, where Aristotle infers 'not necessary' from 'not impossible'. This inference is very difficult to explain otherwise. The inference is based on the sequence of implications (equivalences?) set up by Aristotle in *De Int.* 13, 22^a16 ff., where again μή ἀδύνατον εἶναι entails μή ἀναγκαῖον εἶναι.

6. *A subdivision of contingency.* Having made these distinctions, Aristotle

goes on to say (in *An. Pr.* I, 13, 32^b5 ff.) that possibility has two applications (the expression he uses is *κατὰ δύο τρόπους λέγεται*). On one hand, it is used to describe what generally happens but falls short of being necessary; on the other hand it is used to describe the indeterminate, that which can be 'thus or not thus' without the prevalence of either alternative. Now the distinction plainly has nothing to do with the difference between possibility proper and contingency. Neither of the two uses distinguished by Aristotle covers what happens necessarily. What we have here is therefore a *subdivision of contingency*. Aristotle's use of the expression *κατὰ δύο τρόπους λέγεται* suggests that he is not distinguishing two meanings of *ἐνδεχόμενον* but rather two kinds of cases to which it can be applied (cf. section 13 of the first paper). This is verified by his remarks on the conversion of statements of contingency. He says that in both cases the possible premise can be converted into its opposite premise, *i. e.* '*p* is contingent' into '*not-p* is contingent'. This is trivial in the case of a *p* which is contingent because it is 'indeterminate'. But Aristotle also holds that the conversion applies to contingency in the sense of that which 'generally happens'. This may seem mistaken: if *p* happens generally but not necessarily, we certainly cannot infer that *not-p* happens generally. What Aristotle means is that even in this case *not-p* is neither necessary nor impossible and hence contingent in the sense of his definition. If 'what happens generally but not necessarily' were one of several meanings of 'contingent', Aristotle would not be able to say that 'contingent' always converts with its opposite. What he means is that in each of the different cases that fall under the term 'contingent' we have a conversion to the opposite of some case — not necessarily of the same case — covered by the term.¹ Hence, he is not dealing with different meanings of *ἐνδεχόμενον*, but only with different applications of the term. 'Contingent' is not homonymous although it covers different kinds of cases.

7. *An analysis of An. Pr. I, 3, 25^a37—25^b19.* Some of the passages I have just discussed are referred to by Aristotle earlier in *Analytica Priora* in connection with the conversion of problematic (possible) premises (*An. Pr.* I, 3, 25^a37—25^b19). We are now in a position to understand the context of these references.

In *An. Pr.* I, 3, 25^b18 Aristotle refers to his later discussions of the conversion of problematic premises. All the remarks on this subject later in *Analytica Priora* pertain to contingent premises. This suggests that the notion Aristotle has in mind in 25^b18 is his 'possibility as defined' or contingency. This is confirmed by the way Aristotle explains the notion of possibility which he is here dealing with: "But if anything is said to be possible because it is the general rule and natural . . ." (*An. Pr.* I, 3, 25^b14 ff.). This re-

calls one of the different cases of contingency discussed in *An. Pr.* I, 13, 32^b5 ff. (*vide supra*). And when Aristotle says that this "is the strict sense we assign to possible" (Ross's translation), he is obviously anticipating his definition of contingency in 32^b18—20.

I conclude, therefore, that in *An. Pr.* I, 3, 25^b14—18 Aristotle is thinking of contingency rather than possibility proper. Now the passage which was just quoted shows that this variant of possibility is contrasted to the one employed in the immediately preceding passage (*An. Pr.* I, 3, 25^b8—13). We may therefore expect that this latter notion of possibility is what I have called possibility proper. Aristotle's examples show that this is in fact the case. In one of his examples the term 'man' necessarily does not apply to any horse, while in another the term 'white' does not necessarily apply to any coat. This shows that the meaning of possibility which is used here covers cases of necessity as well as cases of contingency. (The examples are both negative in form because he is discussing the conversion of negative premises.)

Although the testimony of Aristotle's examples thus unambiguously shows that in 25^b8—13 he is discussing possibility proper, one may still be puzzled by his own explanation of the variant of possibility he is using: "Whatever is said to be possible because it is necessary or because it is not necessary admits of conversion like other negative statements" For what one would expect here is 'neither necessary nor impossible' instead of 'not necessary'. Some commentators have tried to emend the passage by inserting the negative particle $\mu\eta$ so as to make it read 'not necessarily not', although there is no real support for such an insertion in the MSS (see Ross, *op. cit.*). Moreover, this insertion has the disadvantage of making the clause 'because it is necessary' superfluous. In any case, the emendation is quite unnecessary, for we have already found independent reasons for suspecting that Aristotle sometimes uses 'not necessary' ($\tau\omicron\delta\ \mu\eta\ \alpha\nu\alpha\chi\alpha\alpha\tau\omicron\nu$) and, by analogy, 'not necessarily' ($\mu\eta\ \epsilon\tilde{\xi}\ \alpha\nu\acute{\alpha}\chi\eta\varsigma$) as elliptic expressions for 'neither necessary nor impossible' and 'neither necessarily nor impossibility', respectively (see section 5, comment (5) *supra*). This suspicion is now confirmed by the fact that the same explanation works here: on my reading the quoted passage says just what one is entitled to expect.

Here one may ask whether my reading is contradicted by the fact that in his second example Aristotle says that it is not necessary that 'white' applies to any coat ($\tau\omicron\delta\ \delta\epsilon\ \omicron\upsilon\chi\ \alpha\nu\acute{\alpha}\chi\eta\ \upsilon\pi\acute{\alpha}\rho\chi\epsilon\iota\nu$). If Aristotle were consistently using the elliptic mode of expression, should he not use double negative $\omicron\upsilon\chi\ \alpha\nu\acute{\alpha}\chi\eta\ \mu\eta\ \upsilon\pi\acute{\alpha}\rho\chi\epsilon\iota\nu$, since he is here dealing with negative premises? To this it may be answered that 'neither necessary nor impossible' is symmetrical

with respect to negation, so that no extra μή is needed even if the elliptic mode of expression is used. Besides, one of Bekker's MSS as well as Philoponus do have the missing μή (see Ross, *op. cit.*), so that Aristotle may very well have been even pedantically consistent in his usage.

We may conclude that in his treatment of the conversion of negative problematic premises Aristotle first discusses premises in which the notion of 'possibility proper' is used and then those in which the notion of contingency is used. In contrast, both these notions are lumped together in Aristotle's discussion of the conversion of positive problematic premises (25^a37—25^b2). He indicates this as follows: . . . ἐπειδὴ πολλαχῶς λέγεται τὸ ἐνδέχασθαι (καὶ γὰρ τὸ ἀναγκαῖον καὶ τὸ μὴ ἀναγκαῖον καὶ τὸ δυνατόν ἐνδέχασθαι λέγομεν) Here the words πολλαχῶς λέγεται suggest that he is not exclusively concerned with the different meanings of ἐνδεχόμενον. In fact, it has been pointed out by Ross that the three cases listed in the parenthetical clause cannot possibly be as many different meanings of ἐνδεχόμενον. However, it seems to me that it cannot be said, either, that they are just three different cases to which the notion of possibility can be applied. The first two are clear; we have encountered τὸ ἀναγκαῖον and τὸ μὴ ἀναγκαῖον before as the two cases covered by the notion of possibility proper. The recurrence for the fourth time of the elliptic expression τὸ μὴ ἀναγκαῖον (or of one of its variants) where one expects 'neither necessary nor impossible' gives further support to my interpretation of this phrase. But τὸ δυνατόν cannot very well be a third case to which the notion of possibility is applied, for there is no third case comparable with the two already listed. Rather, we must understand τὸ ἀναγκαῖον καὶ τὸ μὴ ἀναγκαῖον as referring to the notion of possibility proper, and understand τὸ δυνατόν as referring to the other notion of possibility, *viz.* contingency. This, in fact, seems to be the way Ross understands the passage. Its meaning may hence be expressed somewhat as follows: " . . . seeing that possibility has many applications (for we call possible both that which is necessary or is not necessary either way and that which is capable of being) . . .".

This interpretation is supported by the fact that the context shows that Aristotle is here treating both the variants of possibility at the same time. If they are here mentioned in the same order in which they are subsequently treated (in the connection of the conversion of negative problematic premises) we can scarcely separate Aristotle's references to the two variants in any way different from the one just suggested.

Our interpretation also agrees with the way δυνατόν is used elsewhere in *Analytica Priora*. The most important passage in which this term occurs is *An. Pr.* I, 15, 34^a6 ff. And it is indicated by Aristotle (in 34^a14) that the

arguments he there gives pertain to possibility with respect to generation. Now this variant of possibility is very likely just our contingency. For something which is generated will sometimes be (*viz.* after having been generated) and sometimes not be (*viz.* before it is generated). It is therefore possible in the very sense (in that of contingency) which we wanted to give δυνατόν in *An. Pr.* I, 3, 25^a39.

8. *Remarks on An. Pr. I, 13, 32^b25—32.* What we have found in this paper and its predecessor confirms my earlier analysis of *An. Pr.* I, 13, 32^b25—32 (in 'Necessity, Universality, and Time in Aristotle', pp. 86—88). Here I shall only briefly outline the argument, adding such new evidence as was not mentioned in the earlier paper.

In the passage under discussion Aristotle seems to be saying that

(P) it is possible for A to apply to all B

is ambiguous in that it may mean either

(P₁) it is possible for A to apply to everything
to which B in fact applies

or

(P₂) it is possible for A to apply to everything
to which B possibly applies.

This cannot be his meaning, however. For one thing, he never seems to use (P₁) but only (P₂) in his subsequent discussion of syllogisms from possible premises. He seems even to say that (P₂) is what (P) was *defined* to mean (*An. Pr.* I, 14, 33^a24—25). For another, the term Aristotle uses is δῆχῶς, which strongly suggests that he is not at all distinguishing two different meanings of (P). Rather, he is saying that (P) covers two kinds of cases, *i. e.* that (P) is tantamount to the conjunction of (P₁) and (P₂). This suffices to explain everything that Aristotle says and does. It may be expected that the variant of possibility Aristotle is using in (P₂) is the one he usually employs, *viz.* contingency. Expressed as explicitly as possible, Aristotle's point therefore is that (P) is equivalent to the conjunction of (P₁) and (P₂), where the latter is

(P₂) it is possible for A to apply to everything
to which B applies contingently.

Now this conjunction is clearly equivalent to what we get by assuming that the variant of possibility used in (P₂) is my 'possibility proper':

(P₂₂) it is possible for A to apply to everything
to which B possibly applies (in the sense of

'possibility proper'), *i. e.* to everything to which B applies necessarily or contingently.

This explains why Aristotle seems to deal exclusively with (P₂) in his syllogistic theory; for what he is really dealing with is (P₂₂) which is equivalent to the conjunction of (P₁) and (P₂₁) and therefore also to (P).

Further evidence is perhaps found in *An. Pr.* I, 29, 45^b31—32. Having just explained how the different kinds of assertoric syllogisms are established, Aristotle goes on to say that apodeictic (necessary) and problematic (possible) syllogisms are established in the same way. But he adds a warning:

In the case of problematic propositions, however, we must include those terms which, although they do not apply, might possibly do so; for it has been shown that the problematic syllogism is effected by means of these . . . (H. Tredennick's translation in the Loeb Library edition.)

Prima facie this is completely tautologous. For problematic syllogisms contain by definition terms which do not apply but may apply. What can Aristotle mean here? It is clear that the *predicate term* A of a premise like (P) may apply possibly but not actually. But it is not equally obvious whether the *subject term* B is to be taken to apply possibly or actually; whether, in other words, (P) is to be understood as being equivalent to (P₁) or to (P₂₂). Unless we assume that Aristotle's statement is pointless, we can scarcely interpret it except as a repetition of the point which we found him making in *An. Pr.* I, 13, 32^b25—32, *viz.* as identifying (P) and (P₂₂). Notice in particular that there is no semblance here of a distinction between two meanings.

9. *Concluding remarks.* We have discussed the most important passages of *An. Pr.* I which turn on the distinction between the various notions of possibility used by Aristotle. Insofar as we have been successful in applying the results of my earlier analysis of the ambiguities of ambiguity in Aristotle, our success conversely serves as a further confirmation of the earlier analysis. In particular, it supports what was said in section 10 of the first paper. Aristotle's own definition of contingency (see *supra* section 4) establishes a connection between contingency and possibility proper: contingent is that which is (properly) possible but not necessary. If homonymy were tantamount to the absence of any common element in definition, contingency and possibility proper would not be homonyms. The fact that Aristotle calls them homonyms shows that there is more to his notion of homonymy than that.

NOTE

¹ Aristotle's awareness of the fact that a case of a concept may be converted into another case of the same concept is also shown by his remarks on τὸ ἐκδόξ in *Rhet.* II 23, 1402^a9 ff.

PHILOSOPHY AND THE ANALYSIS OF LANGUAGE

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Both Wittgenstein, in the *Tractatus Logico-Philosophicus*, and Russell, advancing the philosophy of Logical Atomism, maintained that statements are, or purport to be, records of facts. Wittgenstein held that philosophers, by improperly interpreting language, create for themselves pseudoproblems, and that, to avoid confusion, we should throw statements into a form in which their true function, that of picturing facts, would be revealed more clearly and readily than it is in ordinary language.

Russell agreed that the statements of ordinary language should be translated into another form. But for him the reason for such translation was not just that ordinary language, while it functions perfectly well in ordinary life, misleads philosophers, but also, and more important, that ordinary language really gives an incorrect portrayal of facts. And only by translating the statements of ordinary language into a form which *does* reflect facts accurately can philosophical progress be made. For Russell, such progress was not merely (as it was for Wittgenstein) of the negative sort that consists of the elimination of confusion, but of the positive sort that consists of the discovery of new information about facts.¹

In this paper, I wish, first, to consider these two views, showing some of the reasons why they are open to severe criticisms, not all of which have yet been made fully clear; and second, to show how, by dropping or modifying some of the fundamental theses of these two views, certain positions highly influential in philosophy today have arisen.

I will begin my discussion with a study of one of the most famous and influential articles of what might be called the "Transition Period" of Twentieth Century philosophical analysis — the period, that is,

between the *Tractatus* and Logical Atomism on the one hand, and the later views of Wittgenstein and the present views of Austin and others on the other. This article, which even today holds the place of honor in one of our most-used anthologies, is Ryle's "Systematically Misleading Expressions."² My reasons for centering attention on this article are as follows. First, it advances, simultaneously and inconsistently, both of the views outlined above; but in spite of this inconsistency, it advances each of its incompatible theses in a clear and powerful way, making plain the changes which it had to make in the original formulations of them. Second, because of its clearness, it wears its difficulties (and hence those of a whole tradition) on its sleeve, and thus points the way to later developments. Thus, through a close examination of this article, we will be able to survey the whole development of at least one side of Twentieth Century philosophy deriving from the above-mentioned views of Russell and Wittgenstein, and to evaluate some of the strengths and weaknesses of the earlier phases of that tradition.

I

The argument of "Systematically Misleading Expressions" departs from the following thesis:

There are many expressions which occur in non-philosophical discourse which, though they are perfectly clearly understood by those who use them and those who hear or read them, are nevertheless couched in grammatical or syntactical forms which are in a demonstrable way *improper* to the states of affairs which they record (or the alleged states of affairs which they profess to record). (pp. 13-14)

Although such grammatical forms do not obscure from the ordinary man in his everyday business the true meaning of the expressions, to anyone who tries to analyze them closely they present a vicious trap.

... those who, like philosophers, must generalize about the *sorts* of statements that have to be made of *sorts* of facts about *sorts* of topics, cannot help treating as clues to the logical structures for which they are looking the grammatical forms of the common types of expressions in which these structures are recorded. And these clues are often misleading. (p. 22)

Such expressions, Ryle finds, fall into fairly definite groups or classes, each class being misleading in a certain way. Hence they are not simply misleading; they are "systematically misleading," in that they can be classified according to the type of presupposition which, in our attempt

to analyze them, they tempt us to make. Thus in speaking of such expressions he says:

... all alike are misleading in a certain direction. They all suggest the existence of new sorts of objects, or, to put it in another way, they are all temptations to us to 'multiply entities.' In each of them... an expression is misconstrued as a denoting expression which in fact does not denote, but only looks grammatically like expressions which are used to denote. (p. 32)

Ryle lists several types of systematically misleading expressions: quasi-ontological, quasi-platonic, quasi-descriptive, and quasi-referential. Each type is misleading in its own way: what they all have in common is that they mislead philosophers to add entities beyond what the facts really warrant. We might put Ryle's point metaphorically by saying that philosophers are led to add entities "behind" (quasi-ontological), "above" (quasi-platonic), and "along side of" (quasi-descriptive and quasi-referential) entities which are elements of states of affairs. (Ryle incidentally lists two other types of systematically misleading expressions (pp. 32—33), but these do not seem to differ radically from the above, for they too suggest to the analyst a multiplication of entities.)

This trap must and can be avoided; for

what is expressed in one expression can often be expressed in expressions of quite different grammatical forms, and... of two expressions, each meaning what the other means, which are of different grammatical forms, one is often more systematically misleading than the other.

And this means that while a fact or state of affairs *can* be recorded in an indefinite number of statements of widely differing grammatical forms, it is stated better in some than in others. (p. 33)

From these considerations Ryle draws a conclusion pertinent to philosophy.

Such expressions can be reformulated and for philosophy but *not* for non-philosophical discourse must be reformulated into expressions of which the syntactical form is proper to the facts recorded (or the alleged facts alleged to be recorded). (p. 14)

II

Underlying Ryle's argument is a theory of the relationship between language and the world which statements are about. This theory is

a development, often in the same terminology, of certain views which were for the most part due to Wittgenstein, and which were presented in the *Tractatus* and Russell's papers on Logical Atomism. Ryle, however, often gives detail where Wittgenstein gave only bare sketches; he also tries to avoid some of the objections which had been or could be raised against the views of Russell and the *Tractatus*.

For Ryle, as for Wittgenstein and Russell, every significant statement is, or rather purports to be, a record. What a statement records they called a "fact," a "state of affairs," or "(what is) the case," using these expressions interchangeably. (But *cf.*, below, Note 3.)

Developing this Wittgensteinian theory of the relation of language to reality in detail, Ryle states that in order to qualify as purporting to be a record, a sequence of words must fulfill two conditions: (1) it must have certain constituents, and (2) these constituents must be arranged in a certain order or structure. (p. 14) Presumably corresponding to these two conditions of a significant statement, there are two aspects or components of a state of affairs: (1) a "subject of attributes" or several such subjects, together with their attributes, and (2) the "logical form" or "logical structure" of the fact. Sometimes Ryle also speaks as though expressions in general — single words and phrases as well as whole statements — record facts (though he also speaks of "events" in this connection³); but this usage does not seem consonant with his main trend of thought.

Ryle fails to analyze for our benefit the "certain structure" which a significant statement must have in order to be significant; but the constituents of a true statement seem for him to record either the subjects or the attributes, depending on unspecified syntactical factors. To parallel this, we would expect to find him saying that the grammatical form of the statement bears (or ought to bear) some kind of reference to the logical structure of a fact. On this point, however, there is a great deal of obscurity in the article. Certainly Wittgenstein supposed that the logical syntax of language parallels the structure of facts⁴, and held that the structure of facts would be most clearly reflected by some syntax such as that provided by the system of *Principia Mathematica* (*cf.*, *Tractatus*, 3.325). But much had happened to logic and philosophy in the decade since the publication of the *Tractatus Logico-Philosophicus*. For one thing, Gödel's Theorem had done much to shake the confidence of those who idolized logical systems. Furthermore, difficulties had already been revealed in the supposition that everything in ordinary language can be cast into an extensionalist syntax

without gain or loss of meaning; and finally, and most important, the possibility of syntactical systems other than that of *Principia* was beginning to be understood. Why, then, should the grammatical structure provided by *Principia* have any special priority, in that the structure of facts should be represented better or more naturally by it than by any alternative system? Where, indeed, had Russell and Wittgenstein gotten the incredible assumption that the world must conform to the specifications of logic? (No wonder Russell said that "My philosophy is the philosophy of Leibniz"!) Only one step more needed to be taken to reach an even more fundamental question: Why should it not be that the choice of syntax is simply a matter of convention, rather than of the nature of things?

That Ryle was aware of these difficulties is shown in the "consequential" question he raises toward the end of the article: "... is this relation of propriety of grammatical to logical form *natural* or *conventional*?" (p. 34) On the one hand, he does "not see how, save in a small class of specially-chosen cases, a fact or state of affairs can be deemed like or even unlike in structure a sentence, gesture or diagram." (p. 34) For in a fact, he "can see no concatenation of bits such that a concatenation of parts of speech could be held to be of the same general architectural plan as it." (p. 34) Certainly here is a denial of the Wittgensteinian view that "In order to understand the essence of the proposition, consider hieroglyphic writing, which pictures the facts it describes. And from it came the alphabet without the essence of the representation being lost." (*Tractatus*, 4.016) And it makes us wonder about Ryle's own statement, in this very same article, that systematically misleading expressions

must be reformulated into expressions of which the syntactical form is proper to the facts recorded (or the alleged facts alleged to be recorded). (p. 14)

Yet on the other hand, Ryle finds that "it is not easy to accept what seems to be the alternative that it is just by convention that a given grammatical form is specially dedicated to facts of a given logical form." (p. 34) Thus he feels himself unable to reject entirely the idea that there is at least some kind of parallel between grammatical form and the form of facts, though he is unable to describe the exact locus of that parallel.

But not only is the relationship between statements and the facts they record obscure; Ryle's account of "facts" themselves is also

unclear — again, no doubt, an effect of the critical analysis to which that notion had been subjected (it is to be noted particularly that Ryle speaks only of “facts,” never — as did Russell and Wittgenstein — of “atomic facts”). For we are told that “a fact is not a collection — even an arranged collection — of bits in the way in which a sentence is an arranged collection of noises or a map an arranged collection of scratches. A fact is not a thing and so is not even an arranged thing.” (p. 34) Contrast this with the simple (perhaps naïve) view expressed by Wittgenstein in the *Tractatus*: “The configuration of the objects forms the atomic fact” (2.0272); “The way in which objects hang together in the atomic fact is the structure of the atomic fact” (2.032). Ryle has told us that we are not to understand the term “fact” as Wittgenstein did; but he has not told us how we are to understand it.

Sometimes, too, he speaks as though there were only one kind of fact — the kind which we should shape statements to meet — and sometimes as though there were *sorts* of facts, so that even systematically misleading expressions indicate or seem to indicate “sorts” of facts. Indeed, according to some passages, such expressions really only record *real* facts and only seem to record unreal ones. (Cf. the following quotations: “. . . they are couched in a syntactical form improper to the facts recorded and proper to facts of quite another logical form than the facts recorded” (p. 14); “‘Satan is not a reality’ from its grammatical form looks as if it recorded the same sort of fact as ‘Capone is not a philosopher’” (p. 19).)

III

The difficulties of Ryle’s account of the relationship between language and the world, and his failure to define the “form” and “content” of each, leave much to be desired; and these problems were not peculiar to his view, either, but were the common heritage of his tradition. But closer inspection of the article leads us beyond these questions, exposing still deeper ambiguities and more profound problems: problems which are, in fact, among the most profound in Twentieth Century philosophy; for, in essence, they are among the questions over which Wittgenstein, reevaluating the ideas of the *Tractatus*, brooded for a decade.

These questions which we must now ask in reference to Ryle’s views are: Why should he have supposed it necessary, for the theories which he advances, to introduce the notion of facts? Was he correct in assuming that his view of translation — of analysis — requires some such notion?

And finally, if his notion of facts is superfluous, what remains of his theory?

It would indeed be a mistake to suppose that, if a criterion for distinguishing between two classes cannot be formulated explicitly and precisely, the distinction is useless or non-existent. But particularly when the distinction is a fundamental one, introduced partly for the technical purpose of refuting opponents, philosophers cannot afford to let it rest on an uncritical and intuitive basis; for it is the very distinction that must be defended in order to justify the criticisms which it makes possible. And Ryle must often have felt that, for his special kind of translation, which consists of going from a "more misleading" to a "less misleading" form of expression, he would have to formulate some criterion of misleadingness, or, conversely, of propriety of grammatical form: some clear procedure or technique by which we can decide, at least in many cases, whether or not a statement is misleading, and, further, by which we can go about remedying this misleadingness.

One possible view would be that extralinguistic facts serve as the grounds or standards according to which we, as philosophers, strive to model our ways of saying things. In order to serve as such standards, these facts would have to be known clearly, at least to a careful observer — whatever might be meant by "careful." This view naturally assumes the explication of the exact relationship between statements and the facts which they record; only then could we understand what is meant by "modelling" statements after facts.

A great many of Ryle's statements suggest that this is actually his view: "... there is, after all, a sense in which we can properly inquire and even say 'what it really means to say so and so'. For we can ask what is the real form of the fact recorded when this is concealed or disguised and not duly exhibited by the expression in question." (p. 36) But if in some moods he did hold this, at other moments he must have been deeply troubled by the difficulties — discussed at such length in this century — of defining explicitly the "given" in experience. For a large number of other statements suggest that Ryle believes, not that facts are clearly known to an observer and can therefore be used as the criteria of the form into which philosophers ought to throw expressions, but the entirely different view that expressions properly formulated can give us a clue to the form of facts otherwise not clearly known.

... as the way in which a fact *ought* to be recorded in expressions *would* be a clue to the form of that fact, we jump to the assumption

that the way in which a fact *is* recorded *is* such a clue. And very often the clue is misleading and suggests that the fact is of a different form from what really is its form. (p. 19)

... those who, like philosophers, must generalize about the *sorts* of statements that have to be made of *sorts* of facts about *sorts* of topics, cannot help treating as clues to the logical structures for which they are looking the grammatical forms of the common types of expressions in which these structures are recorded. And these clues are often misleading. (p. 22)

Philosophy "must then involve the exercise of systematic restatement. ... Its restatements are transmutations of syntax ... controlled ... by desire to exhibit the forms of the facts *into which philosophy is the inquiry*." (p. 36; italics mine.)

Thus the reader may from some remarks be led to think that Ryle holds that the common man is able to discern the true form of facts, and that only a few overly analytical philosophers are misled by the form in which those facts are expressed — so that with just the proper amount of attention to the facts, the true form of those facts can be made clearly discernible in the grammatical form, and the philosophers' errors avoided; and this is a view quite reminiscent of the *Tractatus*.⁵ But now it appears that Ryle believes, with Russell in his Logical Atomism period, that the true form of facts is to be *discovered*, at least by philosophers, through an analysis of the proper form of expressions; philosophy is again, as it was not for Wittgenstein (*cf.*, *Tractatus*, 4.111), a science.

The philosopher errs, it appears now, not in trying to discover the form of the facts, but in considering the grammatical form of an expression as a clue to the discovery of that form; whereas what would really furnish such a clue (and what should therefore be the object of the philosopher's search) would be the true form of the expression. How, then, are we to find this true form? It is perhaps in line with this new suggestion, that a well-expressed statement would give a clue to the form of facts, that Ryle introduces, at the very end of the article, a new procedure for deciding about the propriety or impropriety of a statement.

How are we to discover in particular cases whether an expression is systematically misleading or not? I suspect that the answer to this will be of this sort. We meet with and understand and even believe a certain expression such as 'Mr. Pickwick is a fictitious person' and 'the Equator encircles the globe'. And we know that if these expressions are saying what they seem to be saying, certain

other propositions will follow. But it turns out that the naturally consequential propositions 'Mr. Pickwick was born in such and such a year' and 'the Equator is of such and such a thickness' are not merely false but, on analysis, in contradiction with something in that from which they seemed to be logical consequences. The only solution is to see that being a fictitious person is not to be a person of a certain sort, and that the sense in which the Equator girdles the earth is not that of being any sort of ring or ribbon enveloping the earth. And this is to see that the original propositions were not saying what they seemed on first analysis to be saying. Paralogisms and antinomies are the evidence that an expression is systematically misleading. (p. 35)

Whether or not a statement is misleading is to be discovered, then, not by an appeal to extralinguistic facts which the statement records more or less properly, but by a direct examination of the statement itself, and the statements which it implies. For example, a statement like, "The wall encircles the city," would, in ordinary contexts, in some sense "imply" the statement, "The wall is of a certain thickness." Now the statement, "The equator encircles the globe," might "seem to be saying" something very like what "The wall encircles the city" says — so that the former would imply "The equator is of a certain thickness" in the same way that the latter implies "The wall is of a certain thickness." But in the equator case, such an implication would contradict (in some sense correlative with the sense in which "imply" is used in these contexts) "something" in the original statement. The only way to escape this contradiction, Ryle tells us, is to see that the original statement was not saying what it seemed "on first analysis" to be saying: that statements of this sort "disguise instead of exhibiting the forms of the facts recorded." (p. 35)

Ryle's new procedure for discovering when an expression is misleading, or when an interpretation is incorrect, offers a more modest prize than we might at first have expected; for we find, now, not what the true form of the expression is, but only what it is not (and hence, not what the true form of the fact is, but only what it is not). But can this new criterion show us, "in a demonstrable way" (p. 14), even this?

According to Ryle, the contradiction between "The equator is of a certain thickness" and "The equator encircles the globe" can be removed only if we realize that the latter does not have the "form" the philosopher thinks it has. Yet why is this the only solution? Could an opponent of a Rylean analysis not say, with perfect consistency, that his

own analysis of the proposition is the correct one — that the form he attributes to the proposition is the one which truly reveals the form of the facts? Of course he would not take such a line in regard to cases like those Ryle gives as examples. But he might well do so in regard to cases which he claims to be more difficult and important; and in such cases, we might well, on the present view of analysis, have a problem as to which interpretation is the one that really gives a clue to the form of the facts.

Certainly some such answer would be what a Plato or a Meinong or a Russell of *The Problems of Philosophy* would want to give in reply to a Rylean critique. If the philosopher does take such a line, he will be maintaining that he really understands the statement (the form he attributes to it really revealing the facts), while Ryle does not; and Ryle will be maintaining that he really understands the statement, while the philosopher does not. Ryle cannot then prove the philosopher's interpretation to be wrong unless he assumes his own to be right. But simply to assert the denial of the philosopher's contention as a premise, and so "prove" the latter's contention to be wrong, will not serve to prove anything. The philosopher may well be wrong; but Ryle's argument, at least, will not show him to be so.

Ryle is at liberty, of course, to begin by assuming that he understands correctly the expressions under consideration. Only then he must abandon a number of claims he makes in the paper. He must, as we have just seen, abandon his claim to be demonstrating something about the true form of an expression; and this entails abandonment of the claim to be demonstrating something about the form of facts. Further, he must drop the claim that "there is, after all, a sense in which we can properly *inquire* and even say 'what it really means to say so and so'" (p. 36; italics mine), and that the propositions supposedly deducible, under the philosopher's interpretation, from the expression in question prove "*on analysis*, in contradiction with something in that from which they seemed to be logical consequences" (p. 35; italics mine). For if we understand the expression to begin with — as we must in order to employ the paralogisms and antinomies method — then we need not "inquire" about the meaning, or go through a process of "analysis" to see that it implies or contradicts some other expression which we also understand. And finally, such an approach would make a sham of the paralogisms and antinomies method: for the involved process of finding a contradiction in the philosopher's interpretation of a statement is utterly useless if we know to begin with that that interpretation is incorrect.

Worst of all, still, is the fact that this approach loses the advantage of proving the philosopher in error; and the desire to keep this advantage lies at the heart of the search for a criterion. Some criterion of proper form of an expression seems needed in order to make Ryle's position really an argument. For we would not be able to tell, without first knowing the real intention of a statement, whether or not considering it to have a certain other intention would lead to contradiction; but the real intention is itself what is at issue. Without some criterion, the paralogisms and antinomies procedure amounts either to mere assertion, devoid of any kind of proof or even disproof, or else to an elaborately disguised case of circular reasoning: assuming the denial of a position in order to prove that that position is in error.

The remedies that suggest themselves, however, seem worse than the disease. To reintroduce "facts" as the criteria of misleadingness would again beg the question if the discovery of the facts (or their form) is the goal. And "meanings" or "intentions," even if there were such things, and we could have access to them, and be sure we had not made any error about them, would not settle the present problem; for they would still have to be "correlated" somehow with facts, and this correlation could itself always be questioned consistently by a determined philosopher.⁶

One might hope to salvage the view by a compromise along Logical Atomist lines, by maintaining that we *do* know the structures of *enough* facts to tell us what the structure of language ought to be; and that, once we throw our statements into this form, we will, by inspection of that form, be able to discover new forms of facts (or, perhaps, the forms of new facts).⁷ But this line is open to at least two objections: (1) it still hinges on successful analysis of the obscure technical notion of "fact," and its companion notions of "constituents" and "logical form" and their linguistic correlates and the nature of that correlation; and (2) there is no guarantee that all or even any undiscovered facts will necessarily comply with the forms of already known facts.

IV

What lies at the root of the difficulties we have encountered in Ryle's position? Are they peculiar to him — mere products of his own confusion of two distinct theories? Or is there something more fundamentally wrong with the theories themselves — something which encourages or even perhaps forces such confusion? And will the difficulties be removed

if some overhauling of the whole approach is made — say, for example, the abandonment of the view of language as a record of “facts”?

In order to answer these questions, let us briefly summarize the doctrines we have examined. Beneath the views of the *Tractatus* and Logical Atomism and the traditions they engendered are three fundamental theses, the first two of which were held in common by Russell and Wittgenstein, but the third of which is different for each.

1. *Ordinary language, if it is not flatly self-contradictory, is at least vague, ambiguous, and misleading, and generally fails to permit clear and accurate expression of what we want to say.* This doctrine is fundamental to the *Tractatus*, despite Wittgenstein’s remark (5.5563) that “All propositions of our colloquial language are actually, just as they are, logically completely in order”: the whole trend of that work, and certainly its influence on others, was along the lines of saying that reconstruction of ordinary language is needed.⁸ It is not too much, indeed, to attribute this doctrine to almost the entire philosophical tradition from Thales on; for the program of traditional philosophy has nearly always been conceived, at least tacitly, as the replacement of ordinary ways of looking at the world — and of talking about it — with a new and more precise one.⁹

2. *For a proposition to be expressed clearly and accurately is for the sentence in which it is expressed to “picture” or “record” facts, or, more precisely, for it to “purport to picture or record” facts (Wittgenstein: to represent a possible combination of “objects”). This picturing relation between facts and sentences has two aspects:*

i. *A “form” of the sentence which corresponds to or represents the “form” or “structure” of the fact.* The “form” of the sentence (which is the true form of the “proposition”) is, of course, not necessarily — not even usually, in ordinary language — the grammatical form; it is rather a “logical” one.

ii. *A “matter” or “content” of the sentence which corresponds to or represents the “constituents” of the fact.* Again, the “content” of a fact or proposition was not necessarily ordinary nouns, pronouns, adjectives, or their factual correlates as ordinarily conceived, but, especially for Wittgenstein, something more fundamental.

This doctrine is a version of the old Correspondence Theory of Truth; and the distinction between “form” and “content” of a proposition did not, either, spring full-grown from the brow of Wittgenstein, but is fundamental to Aristotle’s whole philosophy, and reappears clearly in the logical writings of Leibniz; it probably underlies the old distinction

between “syncategorematic” and “categorematic” expressions, and certainly underlies the Twentieth Century distinction between “Syntax” and “Semantics.” The weighty *a priori* considerations (with which I have not dealt explicitly in this paper) which led Wittgenstein to adopt it and its trailers seem, however, to have been brought into the open first by him.

With regard to the third thesis, Russell and Wittgenstein disagreed. (Whether Russell was fully aware of the disagreement is highly questionable.)

3a. *The function of philosophy is to remove misunderstandings which are the products of linguistic confusions.* (Wittgenstein)

3b. *The function of philosophy is not merely to remove confusions, but, more important, to discover the true form (or forms) of facts.* (Logical Atomism)¹⁰

These, then, are the fundamental doctrines of Twentieth Century analytic philosophy up to the later thought of Wittgenstein.¹¹ In this essay we have examined some aspects of these doctrines, and have seen how philosophers of that period struggled to make them precise: how they tried to specify in just what ways ordinary language is vague, ambiguous, and misleading; and to pin down the relations which a perfect language would have to the world — to literalize the picturing metaphor which they believed must indicate the character of that relationship; and to state clearly what the goal of philosophical inquiry is, and to lay down the methods of achieving that goal. We are now in a position to understand the basis of the difficulties which we have encountered.

If one begins — as Wittgenstein began, and as Ryle apparently began — with Thesis 3a, he will be driven to propose some criterion of misleadingness; merely to assume a certain interpretation of an expression will not suffice to refute a contrary interpretation. To these philosophers, with their picture theory of language, the most natural thing to say here seemed to be that a statement is misleading if it does not reflect the facts clearly and accurately: that is, the facts (Thesis 2) provide the starting-point of philosophy, the model according to which the ideal language (Thesis 2) is to be shaped. But the notion of “facts” proved so intractable that ultimately it could achieve nothing; and these philosophers were forced more and more — like Ryle — to fall back on other criteria (which, however, they still thought of as somehow correlated with “facts”): such were “propositions,” “(real) meanings,” “(real) intentions.” And this policy, of course, begs the question; for, stripped

bare, it tells us that we can find the real meaning by looking at the real meaning. By this procedure, as we have seen, no *proof* is offered that traditional philosophers were mistaken or misled; the assertions made on the basis of circular reasoning remain just that: mere assertions.

If, on the other hand, one begins with Thesis 3b, supposing to begin with that the logical form of the proposition (the real meaning of the sentence) is known, and uses this to determine what the (forms of the) facts are, he falls into similar difficulties. For how do we tell that *this* is the "proper" form of language, the real meaning of the sentence? Surely not by noting that it conforms to the facts! For that would be saying that we discover the facts by examining sentences which are shaped according to the facts. Yet this is exactly the position in which we found Ryle at one juncture.

In short, it appears that whichever aspect of Thesis 2 we take as primary — whether, in line with Thesis 3a, we take facts as our starting-point and the ideal language as our goal, or, in line with 3b, we take the ideal language as known and the discovery of the facts as the goal — we are driven into circularity: we seem forced to assume *both* that we know the facts, and so can discover the true meaning or real intention or true form of the proposition, *and* that we know the true meaning or real intention or true form of the proposition, and so can discover the facts.¹² It is this tendency, arising necessarily from the weaknesses of the two positions, and not a mere mental lapse, that accounts for the inconsistencies which we have found in Ryle's paper, and which, I believe, close attention will reveal in so many of the basic papers of the early and middle periods of the analytic movement.

V

It is impossible not to ask, at this stage, whether the later developments of analytic philosophy have succeeded in overcoming these weaknesses. To deal with such a question is obviously far beyond the scope of any single essay; but a brief account of the course of development, with regard to the above theses, of later philosophical analysis, can serve as a background for further and more detailed investigations.

Some of the most important developments have emerged from a repudiation of Thesis 1¹³: many philosophers now hold that when we properly understand the roles, the functions, the jobs, the uses (let us avoid the difficult and problem-generating term "meanings") of ordinary expressions, we will see truly, as Wittgenstein had seen earlier,

that "All propositions of our colloquial language are actually, just as they are, logically completely in order."

It must not be supposed, however, that the abandonment of Thesis 1 by many philosophers has resulted in agreement among them as to what the function of philosophy is. But behind the many minor differences as to what that function is, two divergent views can be discerned, one claiming kinship with the later thought of Wittgenstein, the other (agreeing with the first group in abandoning Thesis 1) being led by Professor Austin.

(1) This latter group, which really seems to be in the majority at present, will be discussed first, as it is not as much a departure from the views of the early Russell as is sometimes supposed. For, with these philosophers, the "ideal language" of Russell — an artificial construction by philosophers — has been dropped; but in its place, playing a role analogous to that of Russell's ideal language, is ordinary language.

Such a view could, in the light of the criticisms noted in this essay, only be expected to evolve from Logical Atomism: after all,

... our common stock of words embodies all the distinctions men have found worth drawing, and the connexions they have found worth marking, in the lifetimes of many generations: these surely are likely to be more numerous, more sound, since they have stood up to the long test of the survival of the fittest, and more subtle, at least in all ordinary and reasonably practical matters, than any that you or I are likely to think up in our armchairs of an afternoon — the most favored alternative method.¹⁴

Thus, "If a distinction works well for practical purposes in everyday life (no mean feat, for even ordinary life is full of hard cases), then there is sure to be something in it, it will not mark nothing." (Austin, p. 11) It follows that

When we examine what we should say when, what words we should use in what situations, we are looking again not *merely* at words (or "meanings", whatever they may be) but also at the realities we use the words to talk about: we are using a sharpened awareness of words to sharpen our perception of, though not as the final arbiter of, the phenomena. (Austin, p. 8; the qualification, "though not as the final arbiter of," is discussed in Note 15, below.)

The basic error of the Logical Atomists, then, was not in their view that language "marks" something about "realities," but only in their

oversimple view that such “marking” must consist in “picturing.” And because the relation between ordinary ways of talking and “the phenomena,” “the realities,” is not a simple “picturing” one, those early thinkers were led to suppose that ordinary language is imperfect, and so to search for an ideal language. Once we eliminate the naïve mistakes of Thesis 2, therefore, we can deny Thesis 1 and carry out the program of Logical Atomism as expressed in Thesis 3b — remembering, of course, that the word “facts” must not be taken in the way the Logical Atomists took it.¹⁵

(2) In Part I, Sections 89–107, of the *Philosophical Investigations*, Wittgenstein summarizes and analyzes the development of his own earlier views — the views of the *Tractatus*, and, therefore, of the tradition that developed from those views. He explains why he was led — mistakenly — to think of the task of philosophy as the construction or at least the outlining of

a final analysis of our forms of language, and so a *single* completely resolved form of every expression. That is, as if our usual forms of expression were, essentially, unanalyzed; as if there were something hidden in them that had to be brought to light. . . . It can also be put like this: we eliminate misunderstandings by making our expressions more exact; but now it may look as if we were moving towards a particular state, a state of complete exactness; and as if this were the real goal of our investigation. (*Philosophical Investigations*, I, 91).

The essence of language, he thought then, “‘is hidden from us’; this is the form our problem now assumes. We ask, ‘*What is language?*’” (I, 92) And this question — of what is the essential form of language — he wanted to answer by means of the apparatus of “logical form” and “atomic facts” and their accoutrements. “Thought, language, now appear to us as the unique correlate, picture, of the world.” (I, 96) “Its essence, logic, presents an order, in fact the a priori order of the world.” (I, 97) But

the more narrowly we examine actual language, the sharper becomes the conflict between it and our requirement. (For the crystalline purity of logic was, of course, not a *result of investigation*: it was a requirement.) The conflict becomes intolerable; the requirement is now in danger of becoming empty. . . . Back to the rough ground! (I, 107).

In Sections 108—33, he continues explaining his criticisms of these views, and outlines his new procedures. "It was true to say," as he had said in the *Tractatus*, "that

our considerations could not be scientific ones. . . . We must do away with all *explanation*, and description alone must take its place. And this description gets its power of illumination — i.e. its purpose — from the philosophical problems. These are, of course, not empirical problems; they are solved, rather, by looking into the workings of our language, and that in such a way as to make us recognize those workings: *in despite of* an urge to misunderstand them." (I, 109).

In order to bring out the "workings" of an expression, we point, among other things, to examples of its use in actual contexts, and particularly to uses which the philosopher's employment of the expression fails to cover, or with which his uses conflict; we try to show how his problem arises out of his peculiar use of the expression, his use of it in a peculiar context; to show what makes such a misunderstanding not only possible, but seemingly plausible and even appealing; and to show that, once one sees the workings of the expression, all temptation to misunderstand it — to understand it in the peculiar way the philosopher does — disappears, and with that, the philosopher's problem and his doctrine disappear. "The results of philosophy are the uncovering of one or another piece of plain nonsense and of bumps that the understanding has got by running its head up against the limits of language" (I, 119); and this will indeed mean "*complete* clarity. But this simply means," not the achievement of perfect formulation, but "that the philosophical problems should *completely* disappear." (I, 133). Not only will the philosopher's problems dissolve, but also his positive doctrines which are either the attempted answers to or the misguided sources of those problems.

Thus, as Austin continues in the tradition of Logical Atomism, so Wittgenstein carried on the therapeutic conception of philosophy which he advanced in his earlier work.¹⁶ To what extent have these approaches been successful? The present essay has provided a context, a basis, in terms of which a thorough critical examination of them can be made.

NOTES

¹ In sharpening this distinction between the views of the *Tractatus* and Logical Atomism, this essay will be in disagreement with several current interpretations of Witt-

genstein's early thought. Warnock, for example, in expounding the view of the *Tractatus*, claims that "This was in fact closely related to Russell's Logical Atomism; it could be called perhaps a more consistent, more thorough, and therefore more extreme working out of some of Russell's principles and ideas." (G. J. Warnock, *English Philosophy Since 1900*, London, Oxford, 1958, p. 64). Urmson, though he admits that Wittgenstein's early thought was probably different from Russell's (as Wittgenstein himself insisted it was), nevertheless maintains that "it was the sort of interpretation I have given" – of Wittgenstein as a Logical Atomist, – "right or wrong, which was accepted in the period under examination." (J. O. Urmson, *Philosophical Analysis*, Oxford, Clarendon, 1956, pp. ix–x). It will be an incidental purpose of the present essay to argue that the differences between the views of the *Tractatus* and those of the *Philosophical Investigations* are not as great as the Logical Atomist interpretation of the former would suggest; and to show that the influence of the purely therapeutic character of the *Tractatus* was greater than Urmson says it was.

- ² G. Ryle, "Systematically Misleading Expressions," *Proceedings of the Aristotelian Society*, 1931–32; reprinted in A. G. N. Flew, *Logic and Language*, First Series, Oxford, Blackwell, 1952. (Page references are to Flew.)
- ³ Whitehead's metaphysics took events rather than facts as the ultimate building-blocks of reality; and perhaps this accounts for Ryle's otherwise unaccountable introduction here of the term "event," and also for his habitual alternation of "facts or states of affairs": possibly he wanted to surmount the whole problem of facts versus events by relying on an expression which would allow for either solution: "state of affairs" could be construed as *either* "fact" *or* "event," depending on the way philosophers finally decided the question. Ryle's attention would certainly have been called to Whitehead's views by Russell's frequent admiring references to them: Whitehead had explained points in terms of events, and Russell was fond of giving this analysis as a second example (in addition to his own theory of descriptions, which Ramsey pronounced a "paradigm of philosophy") of the technique of logical construction.
- ⁴ "That the elements of the picture are combined with one another in a definite way, represents that the things are so combined with one another. This connection of the elements of the picture is called its structure" (*Tractatus*, 2.15); "The logical picture of the facts is the thought" (*ibid.*, 3); "The thought is the significant proposition" (*ibid.*, 4); "To the configuration of the simple signs in the propositional sign corresponds the configuration of the objects in the state of affairs" (*ibid.*, 3.21).
- ⁵ The *Tractatus* itself does not employ "facts" (or anything else, for that matter) as criteria for discovering what is the correct formulation of propositions. Wittgenstein is there concerned to show what the logical structure of any possible language must be, and, though he holds that some forms of expression would exhibit the logical form of facts more clearly than others, he does not deal with the question of how we would tell which forms of expression are actually clearest. (Probably he would have held that we simply understand expressions, and need no criterion to tell us what they mean; this is a view also suggested by some passages in Ryle, and I will discuss it presently.) For his successors, however, who wanted to apply his views and eliminate some philosophical confusions, the problem could not fail to arise.
- ⁶ Yet another criterion of misleadingness is suggested by Ryle in this paper, *viz.*, Occam's Razor: a statement is misleading if it tempts us to multiply entities. This criterion is not necessarily connected with the "facts" and "paralogisms" criteria discussed above. I will not deal with it in this paper.

- ⁷ In this paper I will pass over the question whether the Logical Atomists believed that the analysis of language would lead to the discovery of the forms of new facts, or of new forms of facts.
- ⁸ *Tractatus* 5.5563 must be understood in the light of such passages as 4.014–4.015, and 4.002: ordinary language is (and must be) as much a picture of reality as any other language; but the “law of projection” which projects reality into language (as a “law of projection... projects the symphony into the language of the musical score”) is an extremely complex one; hence “The silent adjustments to understand colloquial language are enormously complicated.” It is to reveal the picturing relation more simply and clearly that philosophical translation is needed: the picturing relation is there, even in ordinary language (otherwise it could not be a language); hence that language is “logically completely in order.”
- ⁹ Needless to say, traditional philosophers have looked on this doctrine as a conclusion drawn from a detailed analysis of ordinary ways of looking at things and talking about them – not as an assumption made in advance of their thinking. And the critics of the doctrine – to be discussed below – have not just baldly denied it, but claim that detailed analyses of the particular arguments from which philosophers have drawn this thesis, show these arguments to be in error; and that this fact suggests that perhaps the whole doctrine is in error.
- ¹⁰ Some theses of Ryle’s own position have been passed over in this essay: (1) that misleadingness is a matter of degree; (2) that perhaps no sentence can ever be freed entirely of misleadingness (*i.e.*, that the ideal language can never be achieved); (3) that misleading statements fall into definite types or classes; and (4) that misleadingness is not relative to the reader or hearer of a sentence, but is an inherent property of it. I have ignored these theses, despite their considerable intrinsic interest, because they are not of such central importance in the overall historical picture with which I am here concerned.
- ¹¹ It is sometimes said that the “fundamental assumption” (or “mistake”) of these early analytic philosophers lay in the crucial place which they conceived logic to have in the philosophical quest; or, again, that their basic doctrine (and error) was that meaning is naming. But these are merely the trailers of Thesis 2: the crucial importance of logic is asserted by 2i, the doctrine that meaning is naming, by 2ii. All of the theses which I have outlined are central to the positions with which I have been concerned; to maintain that any one of them, or any part of any one of them, is more fundamental than any other, is to falsify the historical picture.
- ¹² When the next step is taken, and we begin looking for a criterion to tell us *both* what is the true form of the proposition *and* what is the true form of the fact, the differences between the two approaches begin to disappear. This fact has increased the tendency of interpreters to conflate the two fundamentally different initial positions stemming from Theses 3a and 3b.
- ¹³ Another very important tradition has refused to relinquish Thesis 1, holding that ordinary language must still be translated into another form. But even for this tradition, this other form is no longer considered to be “correct” in the sense of being a true or accurate representation of the world. Thus Carnap says that “the widely held opinion” that the language recommended by philosophers “must be proved to be ‘correct’ and to constitute a faithful rendering of ‘the true logic’” has led to “pseudo-problems and wearisome controversies” (R. Carnap, *The Logical Syntax of Language*, London, Routledge and Kegan Paul, 1937, pp. xiv–xv). He maintains rather “that

we have in every respect complete liberty with regard to the forms of language; that both the forms of construction for sentences and the rules of transformation . . . may be chosen quite arbitrarily" (*ibid.*, p. xv). The choice of a syntactical system is not to be made on the basis of the "correctness" or "incorrectness" of the system – of whether it corresponds to the "logical form of the facts" – but on the basis of purely pragmatic considerations. And although Carnap in this work explicitly applies his "Principle of Tolerance" only to "syntax," it was not long before the choice of "semantics," too, retreated from the high ground of correctness: the choice, say, between a "sense-datum language" and a "language of appearing" is not to be made on the basis of whether the expressions of one "reach out" to "objects" or "simples" in the world (as Wittgenstein would have had it in the *Tractatus*), but on the basis of "convenience." It was not too long a step from such views to the full-fledged "Logical Pragmatism" of Quine, White, and Goodman; or to the disturbing difficulties concerning "analyticity" and related concepts. But I will not discuss this line of development here; I wish only to point out that it still remains in the tradition of Thesis 1: ordinary language, common sense, is still "vague, cocksure, and self-contradictory," as Russell remarked in *An Outline of Philosophy* (p. 1); and the philosopher's task is still a reconstruction of ordinary language.

¹⁴ J. L. Austin, "A Plea for Excuses," *Proceedings of the Aristotelian Society*, Vol. LVII (1956–57), p. 8.

¹⁵ Austin places two very important qualifications on the use of his technique: (1) we should restrict our application of it to areas of ordinary language which have not been corrupted by philosophical disputes (p. 8); (2) ordinary language must not be considered to be "the final arbiter" of "the phenomena": "ordinary language is *not* the last word: in principle it can everywhere be supplemented and improved upon and superseded. Only remember, it *is* the *first* word." (p. 11)

¹⁶ New reasons are sometimes given in contemporary philosophy for fusing or confusing these two approaches. "What probably happened is this: in the process of dissolving philosophical problems it was gradually seen that certain of these problems arose because of systematic deviations from the ordinary logic of certain concepts. Soon the interest in the logic rather than the deviation became paramount; and philosophy reconstituted itself as a positive, quite autonomous logical activity which is important independently of its ability to clean up traditional mistakes." (M. Weitz, "Oxford Philosophy," *Philosophical Review*, Vol. LXII, 1953, p. 188.) In the light of this interpretation, one might ask whether there is any fundamental difference between the Austinian and the Wittgensteinian approaches. But although there is a sense in which it can be said that Wittgenstein's therapeutic measures are supposed to result in something "positive" (namely, an understanding of the logic of our language), he would never have subscribed to Austin's remarks, quoted above, about the results of philosophy. According to Wittgenstein, we explain our ordinary uses by looking at certain "general facts of nature"; extralinguistic facts are no more revealed by the study of ordinary concepts (uses) than by the examination of any other usable set of concepts. The difference between the two approaches is seen in the fact that, for Wittgenstein, the concepts we choose to examine are not (as for Austin) the ones with which philosophers have *not* dealt, but rather those with which philosophers *have* dealt.

AUSTIN ON KNOWING

by

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Of the many contemporary philosophers who attempt to clarify philosophical questions by investigating ordinary language, perhaps none is more influential than Professor J. L. Austin; and of Austin's published writings, none is more subtle and impressive than his article on "Other Minds."¹ In that paper, Austin proposes to deal with the way in which "ordinary people" answer questions beginning "How do you know . . .;" he begins with questions involved in talk about knowledge of ordinary physical objects, and makes his way gradually to questions about knowing what other people are feeling. In the course of his discussion he very often speaks of ordinary usage, or "correct" usage, of the "implications," the "suggestions," and the various senses of words or phrases in ordinary usage. I want to consider closely some of the statements made about, and arguments based on, common usage, with a view to seeing whether and how they are relevant to the problems of epistemology. It is surely Austin's belief that they are relevant in some way, that he is making clear what is confused in the minds of philosophers, or offering a way out of difficulties. It seems to me, on the other hand, that where he speaks of ordinary language very little of what he says *is* relevant, even if true. I shall not try to comment on all the questions raised by Austin's paper; I wish only to illustrate the *sorts* of statements and arguments which appear there.

The "problem" of other minds is brought on by questions like "How do we know that another man is angry?" Professor John Wisdom (whose paper preceded Austin's in a symposium) asks "Is it like the way in which we know that a kettle is boiling, or that there's a tea-party next door, or the weight of thistledown?" Austin has some doubts about

the accuracy of Wisdom's account of what we would say: "For example, in the case of the tea-party, to say we knew of it 'by analogy' would at best be a very sophisticated answer . . . while in addition it seems incorrect because we don't, I think, claim to *know* by analogy, but only to *argue* by analogy" (p. 148). Now, is it true that "we" don't claim to know, but only to argue, by analogy? A considerable part of his paper is devoted to describing what "we" should say under certain conditions, but neither here nor elsewhere does Austin tell us who "we" are, or how he knows that this is what "we" say. "Ordinary people" helps very little. One can suppose he means ordinary Englishmen, and not ordinary Scots or Welshmen or, certainly, Americans; but this is not precise enough, as even among ordinary Englishmen there must be considerable variation in speech habits. Nor, in case we think him mistaken, do we know exactly where to go to find evidence that would confirm or disconfirm his statements; should we match our own introspection against his, or should we match our own observations of others against his? Or can we, perhaps, merely consult a dictionary?

Austin appears to attribute remarkable subtlety to the "ordinary" usage he describes. He distinguishes, with Leibniz, between cases where one recognizes an object without being able to describe it, and cases where one recognizes and can describe what one recognizes; but he seems to suppose that the difficulties of describing smells, colors and the like are reflected with minute accuracy in "ordinary" language. For instance, he considers the question "How do you know it's a goldfinch?" The answer "'From its red head' . . . differs very materially from 'Because it has a red head', which is also sometimes given as an answer to 'How do you know' and is commonly given as an answer to 'Why do you believe?'" (p. 156). Again, "Any answer beginning 'From' or 'By' has, intentionally, this saving 'vagueness'. But on the contrary, an answer beginning 'Because' is dangerously definite" (p. 157). With Austin's introspective psychology I have no quarrel; but why attach it so firmly to dubious linguistic sociology? Have we any reason whatever to suppose that a majority of people actually do make these very fine distinctions? Yet Austin seems to present the psychological data as evidence for the remarks on language, as though "ordinary" usage mirrored perfectly faithfully the complexities of experience.

We may have doubts about the accuracy of Austin's description of ordinary usage; we may also wonder to what it would be relevant if it were accurate. It might, for instance, be the case that most people (most "ordinary" people) use the word "analogy" as Austin says they

do (though it might on the other hand be true that vast numbers of people never talk about arguing from analogy at all); it would not follow from this that, were we to speak of knowing by analogy, we would fail to be understood perfectly well. If the phrase "knowing by analogy" had never been used, we could introduce it by definition, to mean, say, "knowing on the basis of an argument by analogy;" but of the vast number of grammatically permissible combinations of words which may never have been used, by no means all would be obscure or meaningless if used.

In pressing his claim that we can correctly be said to know propositions which subsequently turn out to be false, Austin says that if we make sure that X is a goldfinch, and it turns out suddenly not to be, then we don't say that we were wrong, we don't know what to say: "Words literally fail us" (p. 160). This is hard to believe, and harder to see as relevant to epistemology. What is to stop us from confessing frankly that we were wrong? Perhaps only the fact that it's correct to be speechless. But if it were true that people were reluctant to admit that they were wrong after trying very hard to make sure they were right, what would that have to do with philosophy? Would it not be a simple psychological oddity? Austin says, one is making no predictions when one says that X is a goldfinch, and in a very good sense one can't be proved wrong. What is a "good" sense of "wrong"? An intelligible one? An established one? There may be a sense of the word "wrong" in which people can *never* be proved wrong, or if there isn't, we can invent one; but it is not one that would interest philosophers, except Protagoras.

It is difficult to see, without more help than Austin gives us, what is meant by a "good" sense of a word; it is just as difficult to see what is meant by saying, as Austin on occasion does, that a word has "no use" in certain contexts. The words "symptom" and "sign" are said to have no use except when we are speaking of a case in which the item itself is likely to be hidden (p. 177). Does this mean that the phrase or word *cannot* be used outside these contexts? Or does it merely mean that it has not yet been used outside? If a word or phrase is used to cover a new case, it may be by metaphorical extension of meaning (but a metaphor can die and pass into "ordinary" language) or by stipulation, but it may be neither of these; the meaning may stay *exactly* the same. If we are told that a word cannot be used outside given contexts, we need to know why: does it lead to nonsense, or to "incorrectness"? If only to the latter, we need to know (what Austin does not tell us)

what are the canons of propriety and why particular incorrect specimens are more misleading than others.

After classifying possible answers to questions like, e.g., "How do you know there's a bittern at the bottom of the garden?" he says, "Among the cases where we give our reasons for knowing things, a special and important class is formed by those where we cite authorities . . . If asked 'How do you know the Persians were defeated at Marathon?' I am apt to reply 'Herodotus expressly states that they were.' In this case 'know' is correctly used: we know 'at second hand' when we can cite an authority who was in a position to know (possibly himself also only at second hand)" (p. 153). This is extraordinary. It is now clear, for instance, that Gibbon was wrong to doubt that the martyrs of Tipasa in Mauretania continued to speak after their tongues were cut out by the Vandals; for he had the story not only from the philosopher Aeneas of Gaza, "a cool, a learned, an unexceptionable witness," who saw them himself, but also from Bishop Victor, who heard it from one of the victims. But of course Austin does not mean, by telling us about the "correct" use of the verb "to know," to claim such an extension of our knowledge: he means only that to talk in such a fashion is correct, is good English, even when the statements one makes are false. This sort of "knowledge" is of course "liable to be wrong" — both expressions are put in quotation marks, as if to indicate that it's not knowledge in some sense, or that we're not really liable to be wrong except in some special sense. Human testimony is sometimes unreliable, he admits, and we may have some special reason for distrusting particular statements. But in general we believe people; "believing persons, accepting testimony, is the, or one main, point of talking" (p. 154). Of course he is right that human testimony is often reliable, constitutes grounds for saying we know; but that people "correctly" speak of "knowing at second hand" is not itself grounds for asserting the reliability of human testimony. That a sentence conforms to "correct" usage is no indication at all that the sentence is true.

Often it is difficult to know whether Austin is speaking of what "ordinary" people would actually say, or whether he is speaking of what it is "correct" to say. It is likewise difficult to understand what precisely is meant by "correct"; this difficulty may be illustrated by the following. In a long section (pp. 161—9) Austin argues against Wisdom's theory that there are some statements about one's own sensations which can't be wrong; his argument turns on the difficulties of recognition, and is stated largely in terms of introspection. In the end he

suggests that Wisdom has overlooked the problems of recognition because of a "tendency to use a direct object after the word *know*" (p. 167). While it is "correct" to speak of "knowing his feelings in the matter," for instance, this is a special expression which does not "justify any general usage" of a direct object. But such a usage would seem not to be *grammatically* incorrect; in many of the senses of "know" distinguished in the Oxford English Dictionary, the verb takes a direct object. For instance (III. 8), "To have cognizance of (something) through observation, inquiry or information." However, assuming that use of a direct object after "know" is incorrect, Austin continues that *therefore* Wisdom presumably means "knowing his sensations" as equivalent to "knowing what he is seeing, etc." But even this won't do: the "what" in "He knows what he's seeing" is "liable to be understood as a relative, 'that which'" (p. 168). This would be a grammatical error, since although "what" *can* be a relative, in this case it is an interrogative. But suppose it is classified as an interrogative: what then? To construe "I know what you are feeling" as containing an interrogative pronoun will, according to Austin, not "lend support" to the bad practice of using a direct object after "know"; but as the sentence was previously given as equivalent to "I know your feelings," it would seem rather to do just that, for both sentences are grammatically correct and we can presumably understand the former, hence also the latter.

Grammatical correctness is not, of course, a sufficient condition for meaningfulness; nor is it, I think, a necessary one. Again, an expression can be both grammatically correct and lucidly meaningful, yet not be in general use; contrariwise, an expression can be in general use without being lucidly meaningful, even if grammatically correct. Uncritical use of an expression, whether in general use or not, can always lead to difficulties, but if Wisdom's were not the general usage, he would not necessarily be involved in difficulties as a consequence. Perhaps what worries Austin is the danger of one's talking like Kantian and Hegelian philosophers, of starting by talking of knowing objects and ending by talking of knower and known being identical. Such a way of talking is rightly to be deprecated, but it is not obvious that by talking "correctly" we shall avoid talking rubbish.

Austin seems to mean by "correct," however, much more than "grammatically correct" or "current" or "approved by writers of style manuals." One suspects that he might be presented with a sentence which conformed to the rules of English grammar and was used by an acknowledged master of English prose, and still reject it as "in-

correct." He mentions the sentence "He knows the town well" as containing a "legitimate" use of a direct object after "know"; so also, presumably, does "He knows London in a way that I'll never know it." But if we write "He knows his pain in a way I'll never know it," this becomes incorrect, a metaphysical proposition to which the sentence about London does not "lend support." Clearly we do not here have to do with grammatical or stylistic correctness, but rather with something like "right description." For two sentences of similar grammatical structure, we may want to give different logical analyses; that is, other sentences proposed as translations may differ from each other and from the originals in grammatical structure. But there may easily be differences of opinion as to what other sentences are adequate translations, differences which may not be resolvable by appeal to standards of "correctness."

Whatever "correct usage" may be, it is not clear what is the crime in deviating from it, and what are the penalties. I think Austin would say that deviation from common or "correct" usage gets philosophers into difficulties, but his illustrations of this are not convincing. Considering the question of reality, he says: in various "*special, recognized ways*" either my experience or some item under consideration may be abnormal, phoney; I may be dreaming, under the influence of drugs, and so on. Doubts about reality are to be allayed by means of recognized procedures appropriate to the case; but the doubts must always have a special basis. There must be some reason for suggesting that it isn't real, in the sense of a specific way or ways in which it may be phoney. Now, "The wile of the metaphysician consists in asking 'Is it a real table?' . . . and not specifying or limiting what may be wrong with it . . . It is the use of the word 'real' in this manner that leads us on the supposition that 'real' has a single meaning . . . and that a highly profound and puzzling one" (p. 159). We should always ask with what "real" is being contrasted, and then usually we shall find a less fatal word to substitute for "real." But surely it is loading the dice to specify that "real" must always be contrasted with "phoney" or "abnormal." Let us take an example, cited in the OED as an example of the philosopher's use of the word "real" (this is from Lotze's *Logic*): "We call . . . an event real which occurs or has occurred, in contradistinction to that which does not occur." There seems nothing wily about this; further, it is clear from all the OED's other definitions of "real" what may be wrong with the unreal, namely, its lacking the defining characteristic of the real. "Real" may be opposed to "phoney" or

“stuffed” or “imaginary” but it need not be; nor does the fact (if it is a fact) that it is usually so opposed make an alternative, defined usage obscure. It is hard to see how in this example the alleged deviation from common usage could be dangerous. Nor is the next example any more convincing.

Wisdom draws a distinction in cases like that of anger between the physical symptoms of anger and the feeling of anger: the outsider can observe the symptoms, or some of them, but can't be acquainted in any direct way with the feeling, which is “private.” It is essential to Austin's argument to minimize this distinction, but he attempts to do so in what seems to me a self-defeating way. “‘Symptoms’ . . . is being used in a way different from ordinary usage, and one which proves to be misleading” (p. 177). Symptoms, he says, are never talked of “except *by way of implied contrast with inspection of the item itself*,” we don't talk of signs of a storm when the storm is on us. Thus it is misleading to lump together all the characteristic features of an item as signs or symptoms of it. In the case of anger, argues Austin, we speak of symptoms of rising or suppressed anger; redness of face, strong language are not signs of anger, they are “manifestations” of it. Symptoms are normally contrasted with the display of anger, not with the man's own inner personal feeling of anger.

Let us suppose that Austin is right about common usage in the present case — although one can easily think of exceptions. Is it true that Wisdom is using the word “symptom” in a new way, in a sense different from the ordinary one? If he were, he might be misled, in case he were also unaware of the different meaning involved. But in fact Wisdom seems to me to be using the word in precisely the sense indicated by Austin as the ordinary one. Austin says: more is involved in anger than just showing the symptoms (in the “proper” sense) and feeling the feeling: there is also the display. Now Wisdom refers to the symptoms (“proper” sense) and the “display” collectively as “the symptoms,” and Austin knows this; there is no misunderstanding between the two. Wisdom is purposely contrasting the symptoms (his sense) with the feeling, and doing it in precisely the way Austin demands, suggesting, what is admitted by Austin, that we never “see” the feeling but only the symptoms. What Austin objects to most strongly, I suspect, is Wisdom's making just this particular distinction, but I can see nothing “misleading” about it. Austin appears to think it so because he supposes there is a “unique” relation between the feeling of anger and the display of anger, a relation which apparently consists in the fact that when

we feel angry we generally feel like showing our anger. He argues that "it is as silly to ask 'What, really, is the anger *itself*?' as to attempt to fine down 'the disease' to some one chosen item ('the functional disorder')" (p. 181). But why silly? Even if Wisdom is in fact doing this (which I doubt), it seems no more silly for a philosopher to ask about the feelings involved in emotions than for a doctor to ask about the visions which follow the use of peyote. "That the man himself feels something which we don't... is... evident enough, and incidentally nothing to complain about as a 'predicament' — but there is no call to say that 'that' (the feeling) *is* the anger" (p. 181). Austin plays his cards close to his vest: he makes quite incidentally what seems to me the point of his whole paper, that there's no problem, no philosophical problem, if one will only talk normally.

But if Austin fails to convince us that philosophers have misused language in the above examples, he also often leaves us in doubt about his own meanings. For instance, when he is discussing "what sort of thing does actually happen when ordinary people are asked 'How do you know?'" he says: "When we make an assertion such as 'There is a goldfinch in the garden' or 'He is angry,' there is a sense in which we imply that we are sure of it or know it... though what we imply, in a similar sense and more strictly, is only that we *believe* it" (p. 149). Austin does not further specify what the senses of "imply" are, and in fact it is hard to see exactly what he means. For instance, "imply" might mean any of the following: I might, in saying A, intend my hearers to understand B as well; I might wish to give the impression that B. Or, when a listener hears me say A, he might immediately conclude that I also think, or am asserting implicitly, B. Or, the implication relation might be a purely logical one between sentences, independent of anyone's intentions or expectations. Or two, or all three, types might be involved at once, and no doubt Austin could think of many more possibilities. In any case, two different senses of "imply" are supposed to be involved, one strict and one not so strict. Now while it might be the case that, in "ordinary usage," the word "imply" is used extremely loosely, in the present case Austin is not describing the use of the word "imply," but using that word to describe ordinary usage. It is not enough to be told that different senses of a word are involved in two statements; we need to know *what* senses.

One main source of difficulty in understanding the philosophic relevance of Austin's arguments is the disinclination he shows to distinguish what are sometimes called "pragmatic" from "semantic" elements. For

example: "How do you know?" suggests that perhaps you *don't* know it at all, whereas 'Why do you believe?' suggests that perhaps you *oughtn't* to believe it. There is no suggestion that you *ought* not to know or that you *don't* believe it" (p. 150). The "suggestions" of these sentences may or may not be "implications" in some sense; they might be what one could call insinuations. While one doesn't question the fact that people often mean much more than they say, why should one suppose that all of the attitudes, insinuations, "implications" and so on that may be involved in particular utterances of a statement are of equal relevance or weight when one is dealing with philosophy? Perhaps Austin does not believe that they are, but if not, he still makes no attempt, here or elsewhere, to sort out the relevant from the irrelevant, the logical and cognitive elements from the idiosyncratic or purely personal, or ceremonial or social elements. This leads him into strange difficulties when he comes to talk of "performatory" language.

Considering the common saying "If you know you can't be wrong," he argues that, if what he has said earlier is correct, we are often justified in claiming to know even when we turn out to be wrong. How then is one to understand the common saying? As follows: one is *prohibited* from saying "I know but I may be wrong," just as one is prohibited from saying "I promise but I may fail" — if you think you may be wrong you "oughtn't to say you know," though you must have some concrete reason to think so, not just the general awareness of human fallibility (p. 170). Austin's interpretation is clearly not the only possible one; we could construe "If you know you can't be wrong" to mean "Necessarily, if you know, you are not wrong." I suspect (with no more evidence than Austin has) that people might generally agree that one can only be said to *know* when what one believes is true. It would indeed be curious for a person to say continually "I know but I may be wrong," but it is not nonsense, nor can one see who is to prohibit its utterance. In easily imaginable circumstances it would be perfectly appropriate. But this will not do for Austin; for him, to say "I know" is a performance, a ritual act, not a description of a mental state. He draws a parallel between saying "I know" and saying "I promise:" both these phrases are ritualistic, like saying "I do" in a marriage ceremony. "We all *feel* the very great difference between saying even 'I'm *absolutely* sure' and saying 'I know.'" What if one turns out to have been mistaken? "We tend to be rather hesitant." We hesitate between saying "You didn't know," "You can't have known," and "You had no right to say you knew." Some philosophers,

he says, tend to say that I should never, or practically never, claim to know anything, and they say this because they are obsessed with the impossibility of foreseeing the future. But this is "mistaken," because first we are often "justified" in saying we know, despite the fact that we turn out to be wrong, and second, it is not demanded, when I say I know something, that I do more than *believe* about the future. "If 'Figs never grow on thistles' is taken to mean 'None ever have and none ever will,' then it is implied that I know that none ever have, but only that I believe that none ever will" (p. 173 fn.). (It is a bit difficult to see in what sense a statement about figs can "imply" a ritual act.) How does all this resolve the sceptical philosopher's doubts about knowing? Suppose the sceptic means by "knowing" that one is perfectly certain, and can give a rational account of one's belief, and in addition that one's belief is true. In this case his doubts are not likely to be resolved when he is told that to say "I know" is a ritual act; other people's rituals are, on this point, irrelevant to him. Austin might say, it is just the use of "know" in his special sense which gets the sceptic into these doubts. But although a sentence used in a ritual could be quite senseless, it could also have a meaning independent of the ritualistic character of given utterances; Austin seems at times to assume that words used in different contexts, or for different aims, necessarily have different senses.

If we are told that a phrase is used to perform a ritual, we may still ask about its meaning. The question is inadmissible for Austin, for to take "I know" to be a "descriptive phrase" is to commit the "descriptive fallacy" (p. 174); that phrase, being ritual in character, "is not describing the action we are doing, but *doing* it." But though one will grant that language is very often used for purposes other than conveying information, is it necessary that a given phrase should always be *just* ritualistic? One need not think so, as one need not think that, whenever ethical language is emotive or hortatory, it is *only* emotive or hortatory. A ritual phrase like "It's been nice seeing you again" may fulfill its role as a terminal pleasantry and at the same time describe one's feelings, and of course may be used in nonritualistic contexts. The phrase "I know" is sometimes used (at least by me) with no (cognitive) meaning whatsoever, and may often be used as a purely social, ceremonial noise (as when we interrupt another, saying "Yes, I know, but . . .," when we not only *don't* know, but believe the opposite). One doubts that it is used ceremonially either always or for the most part; but even when it is so used, it may still have meaning, it may "describe." To

list completely the circumstances in which a phrase is used, the tone, expression and attitude of the speaker, the attitude of the listener, the effects of the phrase on the listener, and so on, is not to exhaust the relevant semantic facts; there is, or may be, also a meaning, a sense. Austin will treat the phrase "I know" as purely "performatory;" yet he will say that if I say "figs don't grow on thistles," I imply I *know* they don't — though I certainly don't imply merely that I say "I know..." On Austin's account, the sentence "He knows" seems to mean "He says 'I know'" (p. 170 fn.); but this surely is not right. Again, to say "I knew..." must mean more than to say merely "I said 'I know...'," but on Austin's account there is no way to express this something more in the present tense. It would be hard to escape from this tangle without abandoning the notion that some phrases are purely "performatory."

The arguments we have discussed are, I think, representative of those which appear throughout Austin's paper. The primary intention of his paper was to tell how "ordinary people" talk about knowing; we are, I feel, entitled to doubt that it actually does so. In addition, it is hard to see how the information given bears on problems that vex philosophers. Austin appears to think ordinary language, whatever precisely he means by this, to be a great deal more unambiguous than I do, particularly where abstract terms are concerned. Ordinary usage, or the language of all the people with whom I am acquainted, seems to me no more precise than is necessary for the uses of daily life; epistemologists, among others, are trying to be considerably more precise than they need to be in daily life. It is indisputable that philosophers, like carpenters, sailors and a great many others, often do make distinctions not made by those in other lines of business, and it is reasonable to ask that philosophical language be translatable into ordinary language, even if at considerable cost in clumsiness. But Austin appears both to think ordinary language to be considerably more subtle than it is, and to want philosophers to be no more subtle than ordinary users of this language. That such a limitation is too costly is, I think, shown by Austin's own writings; one who is talking about implications and senses needs terms more precise than are the terms "implication" and "sense" in everyday usage. If everyday language is rich in distinctions and shades of meaning relating to particular subjects, say, automobiles, it may also be exceedingly poor in regard to others, say, epistemology. Why be content with poverty? (Though Austin admits, in the recent "A Plea for Excuses,"² the relative poverty of some parts of ordinary language, the tenor of his argument seems to me still a general defence of that

language, at least where it is uncontaminated by "back-seepage of jargon" from philosophy.) On the other hand, there seems to be no reason to think that all of the words used by philosophers are being unduly stretched, are in fact being used with new senses. We should perhaps be suspicious of claims that philosophers are talking in unusual ways, and we need to be shown more convincingly how it is harmful to talk in unusual ways. That philosophers, talking as they do, find themselves involved in complicated problems is unfortunate, but it is unsatisfying to be told simply to abandon the problems and talk like "ordinary people."

NOTES

I wish to thank Professor Benson Mates for directing my attention to the problems discussed here, and for his advice and encouragement.

¹ Aristotelian Society, Supp. vol. XX (1946), 148-87 (reprinted in *Logic and Language* (Second series), ed. by A. G. N. Flew, Oxford & New York, 1953, pp. 123-58). Citations are to the original publication.

² Aristotelian Society, Proceedings, n. s. vol. LVII (1956-7), 1-30.

DISCUSSION

ESOTERICISM AND THE DOUBLE AWARENESS

by

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The appearance in this journal of Ingvar Horgby's article on *The Double Awareness in Heidegger and Wittgenstein*¹ marks a notable contribution to an area of interest which might well become the meeting ground for discussion between Continental and Anglo-American philosophers. If it does nothing else, his article should prompt discussion between these two traditions of philosophical temperament and activity in order to mitigate any possible injustices which his thesis does to either camp. This in itself would be a start in the direction of an interchange of ideas and even, perhaps, methods. While not spokesmen, both Heidegger and Wittgenstein represent powerful and philosophically influential figures within the two traditions, and any attempt such as Horgby's to find possible threads of similarity and mutual concern should be welcomed on its own and on heuristic grounds.

Yet one of the difficulties facing anyone commenting on Horgby's thesis is simply that it is too easy to remain confined to a particular tradition and hence, either reject outright or welcome too quickly or uncritically his arguments. Either extreme would seem to be an unwelcome one in that nothing new would be added to the resolution of the conflict already at hand. Perhaps finding a third or "neutral ground" is as difficult a task as bringing an existentialist and an analytical philosopher together to talk "shop". But in some way the construction of this ground would seem to be the first step.

Before attempting this construction, however, it might be well to examine briefly Horgby's thesis on its own grounds before placing it against a critical framework. From this point the task of construction might proceed along the lines of suggesting or sketching a framework

which, if not "neutral", is at least novel, and against which the type of activity Horgby is engaged in becomes enriched.

I

Horgby's argument may be put in the following manner:

Wittgenstein and Heidegger, while obvious philosophical contraries in both method and attitude, are operating from the same premises. Here "premise" must be understood to mean a state of awareness or a special type of consciousness, rather than the explicit postulate of an extended argument. This special consciousness is one dependent upon a special experience for its inception as well as for its distinguishing characteristics which differentiate it from the consciousness of ordinary experience. This consciousness is, in short, the ontological consciousness presupposing an experience of *dass Seiendes ist*. The task of explicating more fully the distinguishing characteristics of this special form of consciousness may then be considered as the task of ontological analysis. Now while Heidegger has given himself over to the task of ontological analysis, Wittgenstein is prevented from performing a similar analysis because of his theory of language (meaning?). This fact, however, does not prevent Wittgenstein from pointing to the existence of a state of affairs which corresponds to the special form of consciousness, even if he is unable to *say* anything about it. Wittgenstein accomplishes this in various ways throughout the *Tractatus* and the *Philosophical Investigations*, the essential principles of his task remaining the same even if there are changes in method and view between the earlier and later philosopher. Hence, the characterization of the "Double Awareness" in Wittgenstein becomes the task of interpreting certain crucial distinctions and statements within Wittgenstein's writings as evidence for the "that"-consciousness.

Questions pertaining to the distinction between "say" and "show" (*Tractatus*), the role of "language games" (*P.I.*), the refutation of solipsism and philosophical scepticism (*P.I.*), and the relation of ethics to the world (*Tractatus*), are seen by Horgby to represent this special awareness in Wittgenstein. In short, Wittgenstein's treatments of these points are only his manner of clearing away pseudo puzzles about the content of ordinary experience in order to fully highlight the genuine or "real" puzzle, i.e., the metaphysical question of *Sein*.

Heidegger, on the other hand, is less concerned to demonstrate the existence of the "that"-consciousness than to accept it as the basis from

which his own ontological analysis proceeds. Hence, Heidegger lays claim to the universality of his ontological analysis, and in this way the standard of comparison between the two philosophers (the *Double Awareness*) is not central to the philosophy of either one. In the case of Heidegger, the standard of comparison is not central to his ontological analysis. In Wittgenstein's case, it is mis-leading to speak of a philosophical center, for his philosophy is in principle an activity.

As Horgby then asks for the "motive power" in Wittgenstein's philosophy, his task is one which sees the *Double Awareness* as the moving force ("the prime mover of his philosophy") of Wittgenstein's philosophical activity. Wittgenstein's task is then seen as the attempt to clarify the ordinary consciousness for the purposes of establishing the "that"-consciousness and its corresponding metaphysical experience.

II

It might be well to begin these comments with an undisputed and, therefore, trivial point. It is simply that the task of interpreting another man's philosophy (or, if you will, philosophical activity) is a difficult one. But if this appears too trivial it might be modified or qualified by stating that much of the difficulty is not always in the texts. It might rather be found in the attempt to seek acceptance for a particular interpretation which deviates from the one accepted in the cultural circle of philosophers clustered loosely around a school, a movement, or simply a way of seeing and solving philosophical problems. This type of thing might be the subject matter of the sociology of knowledge and shall not concern me here. Nevertheless, it is worth pointing out as a possible forecast of the type of difficulty Horgby can expect from the impact of his article on ordinary language analysis.

But I also suspect that in some way I am leading Horgby into these straits — if they exist at all. There are enough explicit comments in his paper to the effect that *Double Awareness* is not central to Wittgenstein's philosophical activity, or that the "that"-consciousness is not the center of what Wittgenstein says, to make Horgby's analysis little more than an interesting investigation into Wittgenstein, the man. This, of course, is an activity of a different nature from that of an interpretation of a philosophical system (or "activity" in this case) and if this is all Horgby is interested in then the difficulties are no more than bogies. But Horgby apparently intends his article to be more than this. What is more important to note is that on whatever basis Horgby

is proceeding, he must nevertheless wrestle with the content of Wittgenstein's philosophy in order to make his points. Hence, whether he intends to offer a new interpretation of Wittgenstein's philosophical activity, or whether he simply wishes to say something about a mental state of a philosopher, i.e., awareness, consciousness, intention, etc., he must still come to grips with the writings themselves. (Although on the latter account he has the prerogative to exhibit statements about the man as he has done.)

In some way the situation stands upon these alternatives:

- (1) We may see Horgby's task as a proposal for a new interpretation of Wittgenstein's philosophical activity, (and quite possibly of ordinary language philosophy).
- (2) We may see Horgby's task as simply an investigation into the pre-philosophical question of Wittgenstein's mental states, personality, or "existential" insights.²

Now it seems to me that there is a possibility of considering a third alternative. One which allows for the possibility of interpretations such as Horgby's to carry the burden of guilt on the critical criteria accruing to alternative (1.), and yet, which allows us to read Horgby as primarily interested in establishing other points such as alternative (2.). The third is simply that Horgby is to be read as doing both of these tasks and yet not constructing the distinction between them sufficiently enough to warrant our decision as determining when he is doing what. This in itself serves as a criticism of Horgby's analysis since it is possible to make a strong case for distinguishing whenever possible those two activities and the types of questions that are indigenous to each. Each activity would seem to require a different set of critical tools as well as different criteria in order to determine the relative success of the ventures. Inasmuch as Horgby seems to be quite consistently involved in both these activities throughout his paper, it may be felt that the distinction is quite irrelevant to the success of his overall venture. Perhaps this is the case. It must be realized that when this distinction is read from the framework of Horgby's views it may be said to already beg the question. Perhaps that is a point to consider in the criticism of criticism. But if nothing more, it shows that pre-suppositions enter into arguments more than most contemporary philosophers would realize or admit. On the other hand, if the set of critical tools arising from this distinction in activities *is in principle* incapable of doing justice to the existential position, then one may in all latitude be prepared to accept

other tools once they are made explicit. It would be prejudicial to claim that there are in principle no other sets of critical tools available, i.e., no other method to determine the success of Horgby's task. Yet one must begin analysis on the basis of some framework and so I engage the one most suited to the task as I perceive it. Again, it is important to note that there may be others more suited to his type of criticism.

All of which does not and should not be taken as a criticism of the latitude by which Horgby performs his interpretation. The question of "latitude" itself is one which needs further explication on the basis of the greater question which asks what kinds of answers philosophy gives, or better, what kinds of questions it asks. This greater question is in fact a crucial one which requires a preliminary answer in order to make way for a justification of attempts similar to Horgby's. A case might be made for applying criteria other than true or false to a particular interpretation if it could be shown that the philosophical intentions of the interpreter are other than apologetic. One might well wonder just what these intentions are in a particular case, and the procedure by which they were made explicit might be the beginning of justification for the analysis in question. On the other hand, the danger involved via misrepresentation in no way requires us to admit that form of latitude either as intentional or simply as the result of bad philosophy. The answer would seem to lie somewhere in-between apologetics and misrepresentation. The type of answer given may in many ways depend on the preliminary answer given to the question of what kinds of questions and answers one expects from philosophy.

But all of this is offered only as a suggestion in defense of the type of activity which I find to be Horgby's task. It is no conclusion. At best it is a reminder to suspend final critical judgment until the race results are in. And finally, what may bring this problem back to the distinction raised before is that it would seem to be a more fruitful program to offer the device of latitude within a well defined area; namely, the philosophy or philosophical activity which is the object of the analysis intending to offer a new perspective or framework from which the philosophy or "activity" may be seen.

III

It is because Horgby is at times involved with both activities and does not mark at what points he is engaged in either that some confusions and problems arise throughout the course of his article. Some of these

problems arise from a confusion of activities, but others arise directly from his examination of the written texts on various points. I will here deal with a few of the points which I find particularly disturbing and in need of further clarification. Among these are:

1. The manifestation of the "that"-consciousness.
2. The question of philosophical motivation.
3. Wittgenstein's "metaphysical" task.
4. The question of the "ideal of language" proposed by Wittgenstein.

Horgby's discussion of the relation between the "that"-consciousness and the manner of its manifestation is quite puzzling when seen against the framework of his own statements on the subject. At times the latter border on contradiction, but may at best be saved by an element of vagueness. The question of how crucial the "that"-experience is to Wittgenstein's philosophical activity is also one that is only vaguely answered.

For example, Horgby says:

"That"-experience and consciousness does not 'underlie' Wittgenstein's philosophy, as it underlies that of Heidegger. It is *not a part* of Wittgenstein's philosophy, although it is expressed in his philosophical writings and is, in my opinion the *prime mover* of his philosophy. [Italics mine]

At another point:

Wittgenstein, on the contrary, descends from his existential "that"-consciousness to the plane of philosophy in order to free philosophy from all claims to state something factually important (and in this negative way expresses the important realm of the inexpressible). Therefore, Wittgenstein, *in his philosophy is not able to express his "that"-consciousness*. [Italics mine]

Now, out of this thicket of statements about "that"-experience (and/or consciousness), prime movers, and descending to the plane of philosophy, I am left with a feeling that something important is being said but am quite unable to see what it is, and therefore, how important it is. What appears to be crucial here is the question which asks how the "that"-consciousness can be expressed, or, if it is expressed at all. At one point we are told that it is not a part of Wittgenstein's philosophy, (the

notion of the "standard of comparison" as it arises later in Horgby's article) but is expressed within it ("...in his philosophical writings..."). This might serve as a point of departure for further possible argumentation; i.e., it is in fact the case that the "that"-consciousness is not a part of Wittgenstein's philosophy? However, to take this line against Horgby may misrepresent his position. We might then have the rejoinder to read Horgby as saying that neither is it (the "that"-consciousness) a part of Wittgenstein's philosophy nor is it even expressed in his philosophy ("... is not able to express his "that"-consciousness."). But if the latter reading is what Horgby intends it becomes difficult to see how he in fact can ascribe or justify his ascription of the "that"-consciousness to Wittgenstein.³

In brief, the difficulty I find throughout Horgby's use of the terms, "that"-consciousness, "that"-experience, etc., is simply that we must either accept the "that"-consciousness as not revealed (expressed?) in Wittgenstein's philosophy and hence must be silent philosophically, or else we may accept as a point of departure the view that the "that"-consciousness is a part of the content of Wittgenstein's philosophical writings. On the second alternative we can go about making a case for this interpretation. On the first alternative we must direct our interest to Wittgenstein, the man, his personality, friends, and second-hand reports about his behavior. This too would be "making a case" but it seems apparent that the two activities are logically and methodologically distinct.

Yet it would seem that in spite of Horgby's statement to the effect that the "that"-consciousness is not expressed in Wittgenstein's philosophy, that he has gone ahead under the assumption that it is so expressed. (And there is in fact a statement to this effect as well.) What is central to the confusion generated by the vague relation between the "that"-consciousness and the philosophical writings themselves is simply that Horgby is involved with both tasks at once. The conflicting statements are then representative of his dual and conjoint interests.

The question of philosophical motivation may also be raised at this point. This question arises out of the use of such terms as "motive", "motive power", "intention", and "prime mover". (I take the latter to mean something similar to a strong motivating force.) Unfortunately, Horgby, by the use of such terms, commits himself to more naive psychologizing than is usually necessary among philosophers. Although I would again admit that this point already begs the question from the existentialist's standpoint, I suggest that there might be something gained

in the way of clarity by using these terms in their sophisticated psychological senses, or, at the minimum, making explicit the sense in question. That the free-wheeling use of these terms adds even more confusion to the question of the appearance of the "that"-consciousness is apparent when one considers that the earmarking of this special form of consciousness within Wittgenstein's writings is something closely akin to the process whereby latent dream content is explicated by an analysis (psycho) of manifest dream content. If one chooses this form of esotericism for philosophical interpretation then there can be no argument. The real question is, however, whether this is a proper form of philosophical interpretation. Hence, there is some danger in the commitment to the analysis of "motive power" behind a philosophy if this task draws its importance from the minimizing of the importance of the manifest content. In the case of Wittgenstein this is a danger which is compounded by the contemporary appeal of the manifest content. In another way, the danger involved in minimizing the manifest content is that one indirectly loses the chance to offer an alternative interpretation of that which is explicit. With Wittgenstein and the analysis of ordinary language it becomes a real danger to minimize the importance of therapeutic analysis and the analysis of language games in their own right in favor of the task of motive ascription. Not only does one lose the chance to argue against the position of therapeutic analysis on certain issues, but more importantly, the sense of the broader framework from which ordinary language analysis may be seen is obscured. I will have more to say about this latter point. For the moment, however, may it suffice to say that the explication of this broader framework arises from an extension of ordinary language analysis. In no way, moreover, does this framework depend upon motive ascription for its construction or justification.

If it is then the case that Horgby is committed to both types of activities throughout his article, and as I have tried to show, the obfuscation of the distinction between interpretation and motive ascription is a central factor in weakening the thesis of the *Double Awareness*, it seems only fair to inquire further as to what points Horgby has devoted himself more fully to in what may be called the "manifest content". By examining briefly some of these points, it may be possible to see if the notion of the *Double Awareness* may be substantiated by attention to the text itself.

By way of characterizing Wittgenstein's task as primarily a metaphysical one (in the sense that by delineating metaphysics he is supposedly doing

metaphysics), Horgby early in his article relies upon the notion of "experience" in the *Tractatus* in order to introduce his characterization of Wittgenstein as in possession of a special consciousness.

For this a special consciousness is required. Wittgenstein says: '... that something is; but, that is no experience.' The consciousness of *Sein* that something is, is not a consciousness of something.

This fundamental question of metaphysics ... [Italics mine]

Now, besides the danger involved in incomplete quotations (in the direction of misrepresentation — the case of the *Tractatus* radically compounding this danger), I think it is necessary to point out that even upon an alternative reading of this and other crucial paragraphs, Horgby's thesis may in principle remain undisturbed. That is, if he is again to be taken as talking about "consciousness". Hence, no matter if we continue the incomplete quotation ...

The 'experience' which we need to understand logic is not that such and such is the case, but that something is; but that is no experience.

Logic *precedes* every experience — that something is *so*.⁴

... and in opposition to Horgby conclude that Wittgenstein is here making a logical rather than a metaphysical point, we are still faced with the possible rejoinder that whether it is logical or metaphysical is irrelevant to Wittgenstein's consciousness of *Sein*. Here once more we would be tacitly acknowledging the unimportance of the explicit for the purpose of the esoteric.

Indicating the possibility of the argumentative irrelevance of the "logical" reading of this paragraph is not to say that Horgby would consider the alternative reading irrelevant to his thesis. I don't wish to attack a straw man. The point worth making here is, however, that upon this *type* of esoteric analysis we can often slip from the explicit to the latent without making a strong enough case on the evidence which is public and published. At that point it becomes difficult to see just what would count *against* a particular thesis or interpretation.

Yet I think Horgby has made an interesting point in his reading of the quoted paragraph. As he insists upon the metaphysical consciousness of Wittgenstein he jolts our thinking on the question of metaphysics itself, and in this way, his point serves a heuristic purpose. By denying

that Wittgenstein is doing metaphysics in the "news from no-where" fashion and yet sensing that Wittgenstein is doing more than simply making a logical point⁵ ("logic precedes every experience"), we may come to the perspective which sees Wittgenstein's task as one of descriptive metaphysics. This is a framework that has only recently begun to be sketched and itself requires a good deal of justification.⁶ It might be noted, however, that the explication and defense of this view may be located outside of Wittgenstein's writings. If he is included, the task of interpretation will rest upon the content of his philosophy and not upon vague references to his special states of consciousness.

The final critical point which I will take up is the one which rests upon Horgby's analysis of the "ideal of language" employed by both Heidegger and Wittgenstein in order to communicate in language what they have experienced in the "state" of *Double Awareness*. I consider this a crucial point in Horgby's task for it not only highlights a distinction between the "earlier" and "later" Wittgenstein, which is more important for mitigating Horgby's claims than he acknowledges, but also, points to the *Philosophical Investigations* as a text more susceptible to a reading different from Horgby's.

Horgby says:

Heidegger, who does not have Wittgenstein's (*Tractatus*) narrow ideal of language, says much about the characteristics of the real existence . . .

Now, this question of the "real existence" and the "narrow ideal of language" placed against Horgby's perception of Wittgenstein's position in the *Tractatus* deserves a great deal of closer scrutiny. I believe it would be folly to say that Wittgenstein's "ideal of language" in the *Investigations* would in any way condone the "Turkish music" ideal of Heidegger. Horgby himself may be prepared to admit this but the point requires mentioning anyway. It may be put simply and cogently by asking if Heidegger's "ideal of language" is any *wider* in terms of non-truth functional description, performatory usage, or any other mode of sense-making. This seems to be the manner by which Wittgenstein broadens his original "ideal" and in one way refutes the logical atomist (himself) with the indirect suggestion throughout the *Investigations* that language serves other functions than the truth-functional one.

However, it seems only proper to inquire whether Heidegger's "ideal"

is any broader in any of these senses, or rather, if it only proposes to be wider in the descriptive truth-functional sense. It might be argued that Heidegger is *showing* us something in a strangely metaphorical, perhaps poetic style or sense. This might be worth arguing for to the philosopher of ordinary language. But the present question is one which asks if Heidegger is not in actuality operating with the same "narrow ideal" as specified by Horgby, but is also in fact, continually ignoring the limits set on *that form of discourse* by Wittgenstein. (And one must not forget that Wittgenstein too wrote in the German language.)

Again, in relation to Horgby's argument, I find myself a bit confused by his reference to the role of the "habitual language" hiding the "real structure of the world". And further, the notion that, "if Heidegger would have written in ordinary language, he would have abandoned his philosophy". Now the latter statement is one with which the ordinary language philosopher might well agree — for obvious contrary reasons. But while the philosopher of ordinary language would recognize the limitations of the "ideal of language" found in the *Tractatus*, he would at the same time insist upon the richness of ordinary language in that it is capable of expressing clearly anything the philosopher wishes to say — if he says it non-philosophically. While at one point Horgby interprets Wittgenstein's insistence upon the richness of ordinary language ("that the language is an expression of man's attitude towards life, that it is always 'polluted' with existential suppositions, that it is never a sterile organ, that it is a consequence of the general picture which the double consciousness provides"), he is at the same time at great pains to justify the verbal gymnastics of Heidegger by stating that the "usual language" expresses only *das Verfallensein*. Hence, Heidegger must overcome the limitations of ordinary language into which his philosophy could not and cannot be placed.

All of this is quite vague. What appears most puzzling is the use of such terms as "ordinary", "habitual", "usual", etc., in such a way as to suggest inadequacy and sterility when the argument is for Heidegger's escape from the "shackles" of ordinary language, and yet, to suggest that there is an element of richness and conviction when Wittgenstein's position is discussed. Part of the difficulty and the inconsistency may lie in the notion of "ordinary language" itself. A case might be made for saying that Heidegger has not left ordinary language but has only exploited its richness for non-kosher purposes. More careful attention to Wittgenstein's task in the *Investigations* may be one way to alleviate this difficulty.

IV

And finally, the few passing hints and references to the possible perspective of descriptive metaphysics which I find a possible alternative to Horgby's esotericism, and which, by way of contrast may be justified without biographical details, may be subsumed under the suggestion that more careful attention be paid to the latter parts of the *Philosophical Investigations* in which Wittgenstein's expression, "form of life", takes on added meaning when seen against the background of the relation between logic and life. A closer scrutiny of some continental writings, particularly those of the phenomenologists, may also shed some light on this relation. This area as well as the related problem of the "exhibitive" use of language⁷, i.e., where language is used for the purpose of *showing* us something, could well become an important point of departure for Anglo-American-Continental discussion.

NOTES

¹ *Inquiry*, Volume II Number 4 - 1959.

² This may in fact still be considered philosophy but I would prefer to call it existential psychoanalysis.

³ Considering some quotations from personal friends of the philosopher as not sufficient evidence to make a philosophical case in point.

⁴ *Tractatus Logico-Philosophicus*, 5.552, 6.1222, is also quite relevant to this point.

⁵ "Simply making a logical point" may be a misleading way of speaking since it is such points that so often are forgotten or abused. Yet, there is room to read Wittgenstein in another way here. See in particular 6.13 (*Tractatus*).

Logic is not a theory but a reflexion of the world.

Logic is transcendental.

Making a point about logic is thus not "simply making a logical point".

⁶ See in this connection some recent attempts to construct such a framework:

Virgil C. Aldrich, "Chess Not Without The Queen", Proceedings of the American Philosophical Association. Presidential address to the Mid-Western Philosophical Association, 1958.

M. Masterman, "Metaphysical and Ideographic Language", British Philosophy in the Mid-Century, Ed. C. A. Mace.

Strawson, P. F., *Individuals*, Methuen and Co. London, 1959.

⁷ Aldrich, Op. cit.

THE ADEQUACY OF LANGUAGE¹

by

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The notion of linguistic *adequacy* (the adequacy of sentences to express or describe) is explicated in terms of a set theoretical model of the communication situation. Roughly: a message is adequate to the degree it answers the receiver's questions. Adequacy is distinguished from openness, in such a way that a message can be both completely adequate in a communication event and also "inexhaustibly open". Using this explication it is possible to translate and clarify several familiar philosophical theses concerning the adequacy of language. The view that "humanistic" language differs from scientific language is clarified and criticized. Seven types of empirical test of the explication are discussed.

The Problem

A major obstacle to the introduction of quantitative concepts into the humanities is an unclear belief about the adequacy of language. In this paper I shall present an explication of some concepts of empirical semantics, in order to clarify and reconstruct this belief about language.

The view in question takes many forms, from the Whiteheadian view that no verbal formulation can express a proposition adequately to the Wittgensteinian suggestion that there are many types of adequacy.² This view is to be distinguished from complaints about more specific defects of language, such as particular vagueness, ambiguity, vocabulary deficiencies, or the non-translatability of one language into another. The view in question here is typically behind the complaint "Words cannot describe this situation" or "Words cannot express what I have in mind." The intuitive accompaniment of these words may be a picture of some words going out part way, but failing to reach far enough, or failing to spread out wide enough, as though denotation were like a light-ray or a liquid.

Usually present with this view is the additional view that scientific statements differ from non-scientific ones in that scientific statements are adequate expressions while non-scientific statements always or

often have some surplus meaning which the words do not express. This difference is sometimes explained by saying that science expresses mere abstractions while the non-scientific (e.g., literature) deals with life. The operational consequence of this additional view is that humanists deny the relevance of scientific concepts and techniques to humanistic studies.

In this paper I shall show specifically that:

- (1) there is a clear sense in which language is inadequate in some situations, and
- (2) the adequate-inadequate distinction is not correlated with the scientific-nonscientific distinction, and
- (3) the adequate-inadequate distinction need not be an obstacle to the introduction of quantitative concepts into humanistic studies.

Approaches Toward a Solution

The traditional approach to these matters, dating at least from Plato, is by way of dialectical arguments. Against the view that language is inadequate one can use the Wittgensteinian retorts: To allege inadequacy presupposes a standard of adequacy, so language must be adequate in at least some cases. Further, what sense does it make to say that we "approach" adequacy if we know we can never reach it? Again, language might be prevented from being adequate if it were not under our control, but it is under our control, so if we intend that a verbal expression be adequate it will thereby be adequate. Finally, what we intend is that an expression be adequate for a certain purpose, and so it is pointless to criticize it for being inadequate for some other purpose.³

For the view that language is inadequate, however, one might reply: We know that there is a difference between a scientific analysis, which comes to completion in statements which are explicit and unambiguous, and a piece of literary criticism, which does not claim to exhaust the content of the work of art. Further, it is the very intention of the artist to say new things, over and beyond what the rules of language explicitly provide for, and certainly beyond what any user of the language has said before. And, what should be convincing, Gödel's discovery that the class of provable theorems of arithmetic does not exhaust the truths of arithmetic shows that even in mathematics there is surplus meaning over and beyond what can be provided for in explicit rules.

My main assumption here is that dialectical arguments by themselves are more harmful than helpful. By themselves they seem only to make each side feel that it has been misunderstood by the other. Dialectical arguments neutralize each other and thereby reduce to some kind of absurdity a certain set of philosophical techniques. These arguments, however, can be useful as a preliminary to the main task, the task of constructing a viewpoint which will satisfy both sides at least in the sense of preserving what is valuable in both sides.

The task specifically is to explicate our concept of the expression-situation and our notion of adequate expression. The following seem reasonable as guiding requirements for this explication: (1) It should avoid metaphysical presuppositions (e.g., concerning the existence of propositions, the nature of mind, the meeting of minds, etc.). (2) It should avoid presuppositions about types of language or discourse; specifically it should avoid initial commitment to any distinctions between prose, poetry, science, and metaphysical discourse. (3) It should answer or clarify specific questions about the adequacy of words to express feelings or describe the world. (4) It should be applicable to single language events. (5) It should be applicable to many types of language event. (6) It should yield a quantitative concept of adequacy. (7) It should clarify questions about the distinction between scientific and non-scientific language. (8) It should be self-referentially consistent. (9) It should be testable.

Proposals

Let us take seriously the notion that adequacy is always adequacy for some purpose; and let us also assume that purposes are specific, limited, finite (this notion is made precise below, in terms of a finite list of questions).

Let us subsume both semantic and pragmatic adequacy under the more general idea of communicative adequacy.⁴ A justification for this is that we can schematize the communication situation in terms of a message *M* which is more or less adequate to evoke in the receiver the response desired by the sender, and when a person says "I can't describe *X*" or "I can't express my feelings about *X*" we can say that his words expressed by himself as sender fail to evoke the right response in himself as receiver. Another justification is that we thereby avoid commitment to an analysis in terms of semantic, pragmatic, etc., rules, which might seem unduly artificial.

Also, let us distinguish "The words X are inadequate to express a certain meaning" from "The meaning of X is inexhaustible." One justification here is this: The feeling that X is inexhaustible seems to come mainly from the observed fact that no "content analysis" or literary criticism of X is final or complete; but then we may as well ascribe the depth or inexhaustibility to the process of analysis, rather than to X. If we take purposes to be finite, then we can hold both that X is adequate for some purpose and that the content analysis of X is further extendable.

Let us schematize the communication situation in the following way:

A sender sends a signal to a receiver R at a discriminable time t . By some means or other, R interprets the signal as a message M. Regardless of what mood the message-sentence is in (declarative, imperative, etc.) we can regard the message interpretation as issuing in a derivative declarative statement, such as "The sender wants me to believe so and so, or do so and so, or etc." Alternatively, we can simply confine our analysis here to the case of message-sentences in the declarative mood.

The receiver at t articulates his interests by making and asking a finite list Q of questions q_1, \dots, q_n . Each question is of the form "Is it the case that p ?" An answer to a question is always positive, and is of the form "It is the case that p ." Some of the questions concern the subject-matter of the message, others concern the communication situation itself, etc. The receiver also has at t a weight function W by which he assigns to each question a positive number; this number represents a subjective evaluation of the importance to him of receiving an answer to the question. How these numbers are to be assigned and what exactly they represent are irrelevant here; they might be information, urgency, or utility measures, or some function of several of these.⁵ The question list must include both positive and negative forms of the "same" question (that is, if Q includes "Is it the case that p ?", it must also include "Is it the case that not- p ?"), and the number assigned as the value of the positive form of a question may be independent of the number assigned to the negative correlate of that question.⁶

The receiver R also has at t an available fund of knowledge which can be regarded as a class K of assertions. This is R's fund of information which he had prior to receiving M, or which he regards as not due to his knowing what M asserts.⁷ M will be informative to the extent that its content is not contained in K ; hence, if the receiver

desires a precise evaluation of the informativeness of M, he will be motivated to keep M and K clearly separated.

In addition, R has a logical system D. This includes deduction apparatus of some kind, at least in the form of a disposition to give particular reasons for holding particular beliefs. We must assume here that K and D are consistent, in the sense that it is not the case that all statements whatsoever are deducible from D or from K by way of D.

When he receives M, R scans his list Q to see which of his questions can be answered by M. More accurately, for each q he attempts to deduce either p or not-p by means of D from the conjunction of M and K. Let us suppose that of the n questions in Q only m are such that R can at t deduce answers to them from M and K by means of D, where R cannot deduce these answers by means of D from K alone. Let us denote by 'A' the class consisting of these m questions. Some of the deducible answers will be answers to the positive form of a question, and some will be answers to the negative form. Let us call the positive form of a question the 'correlate' of the negative form, and likewise the negative form of a question the 'correlate' of the positive form. Finally, let us denote by 'Q*' the class of questions q such that q is in Q and q is a correlate of a question in A.

Now let us adopt the following definitions:

D1: A communication event for R at t is an ordered set (M, K, D, Q, W) such that R accepts K, D, and W at t, receives or examines M at t, and asks Q at t (that is, asks at t the questions in Q at t), as explained above. This must be an ordered set, to avoid difficulty where M is the same as K. We shall occasionally hereafter drop the reference to R and refer to a communication event simply by 'E_t'. We shall use 'Q_t' to refer to the Q class in E_t, and similarly for K, D, W, A, and Q*. Occasionally we shall drop the reference to t.

D2: A communication sequence for R between t₁ and t_p, where p is either the same as or later than 1, is a set of communication events for R which includes an E_{t₁} and E_{t_p} and possibly other communication events at times between t₁ and t_p, but no others.

D3: A communication analysis for R between t₁ and t_p is a communication sequence between t₁ and t_p such that all the communication events of the sequence have the same M and D, and each K and each Q is included in all later K's and Q's, respectively.

D4: M is adequate in E to the degree N if and only if N is the ratio of $\sum_A W(q_i)$ to $\sum_{Q-Q^*} W(q_j)$.

That is, adequacy is the ratio of the weight of the answered questions to the total possible weight of all the questions. Q^* is subtracted from Q in order that N can have values up to one. When N equals one, we shall say that M is completely adequate. When N equals zero, we shall say that M is completely inadequate.

D5: M is adequate in a communication analysis between t_1 and t_p to the degree N if and only if M is adequate in E_{t_p} to the degree N .

D6: q is latent in Q_t for R at t if and only if there is some subset Q' of Q_t such that an answer to q is deducible by R at t from a consistent conjunction of K with answers to questions in Q' , using D .

D7: M is open in the communication event E for R at t if and only if R asks at t a question q such that (1) q is not latent in Q_t and (2) R can deduce the answer to q from M and K , using D , and (3) R cannot deduce the answer to q from K alone, using D .

D8: A communication analysis CA between t_1 and t_p is endless for R at t_p if and only if R can associate with every time later than t_p a Q such that each Q beginning with Q_{t_p} is included in all the Q 's associated with times later than t_p , and each Q after Q_{t_p} contains some question which is not latent in any earlier Q . That is, R is here required to show how to construct an endless series of Q 's, each Q containing its predecessors but being richer than they.

D9: A potential communication event for R at t is an ordered set (M, K, D, Q, W) such that R accepts K and D at t and is capable of accepting W at t , receiving or examining M at t , and asking Q at t .

D10: M is potentially open in the communication event E for R at t if and only if E is a potential communication event for R at t and R is capable of asking at t a question q such that (1) q is not latent in Q_t and (2) R can deduce the answer to q from M and K , using D , and (3) R cannot deduce the answer to q from K alone, using D .

D11: M is inexhaustibly open for R at t if and only if M belongs to a communication event E for R at t such that (1) E belongs to some communication analysis which is endless for R at t and (2) for every Q_x in the Q series required in D8 (above) M is potentially open in (M, K, D, Q_x, W) for R at t .

Notice that the above definitions have been formulated in such a way as to make analysis and proof in particular cases relatively easy for R , in that he has to deal only with his own D , K , Q , and W . On the other hand, if R wants to prove adequacy or openness, the burden of proof is on him to show that he is capable of the required

constructions and deductions. If R declares M open for himself, a sceptic might be able to perform the deductions R was incapable of, or he might exhibit a D such that M is not open, and the rhetorical effect in this case is that the openness is shown to reside not in M but in R.

The best strategy for justifying these definitions is to exhibit a few consequences and applications in such a way as to answer some of the important questions on these matters. This procedure is in any case required for showing self-consistency, because adequacy has been defined here in terms of answering questions.

Consequences and Applications

(1) A logically true M (that is, one which is deducible from D alone) is completely inadequate in all communication events. Similarly, any M which is deducible from K via D is completely inadequate. Reason: D4.

(2) A meaningless M (that is, *inter alia*, an M such that nothing can be deduced from the conjunction of it and K via D which cannot be deduced from K alone via D) is completely inadequate in all communication events. Reason: D4.

(3) No meaningful M is both (1) not deducible from K via D and (2) completely inadequate in all communication events. Reason: We can always construct a Q which includes "Is it the case that M?"

(4) A contradictory M is completely adequate in all communication events in which D includes a rule or theorem to the effect that any assertion follows from a contradiction. Reason: D4.

(5) Every question whose answer is logically true is latent in Q in every communication event in which D includes a rule or theorem to the effect that any assertion implies a logically true assertion. Likewise for every question whose answer is deducible from K via D. Reason: D6.

(6) If some poetic M is meaningful and not deducible from some K using D, then there is a communication event in which M is completely adequate. Reason: We can construct a Q which includes only "Is it the case that M?" (Similarly for M in scientific or any other type of language).

(7) There is for some R at present a scientific M, and also a humanistic M, each of which is completely adequate and inexhaustibly open. Reason: I myself am considering the message "A new computer, Alpha, is programmed to write an unending list of zeros." This

message is scientific in the sense that it could belong to the natural history of computers. It is completely adequate relative to the question "Is it the case that Alpha is programmed to write something?" (It is completely inadequate relative to the question "Does Alpha enjoy her work?"). It is inexhaustibly open for me now because I can do the following: Since times are discriminable and there are therefore only countably many times, I associate with every time t_n later than now a Q which includes the question "Is Alpha programmed to write a zero in the n -th place?" For W I choose the trivial function which assigns to each q the value one. In addition I can ask and answer a question which is not in Q_{t_n} , namely, "Is Alpha programmed to write a zero in the $n+1$ st place?" To answer these questions I need M as well as D and K , because the question refers to Alpha, and my only evidence for the existence of Alpha is M . The question about the $n+1$ st place is not latent in any earlier Q because no earlier Q contains any question which mentions an unending list of zeros or the $n+1$ st place; I am careful, that is, to keep M separate from K and to include in my Q 's only the above questions, namely "Is Alpha programmed to write something?" and "Is Alpha programmed to write a zero in the ... place?"

I can reason analogously for the humanistic message "The Anti-christ sees the glory of the devil by contemplating an unending list of zeros."

(8) There is a quantitative concept which is useful in humanistic studies. Reason: Consequences (6) and (7) above use $D4$ and they belong to humanistic studies.

(9) The definitions given above can be used to clarify the philosophical theses with which we began, as follows:

"I haven't fully expressed what I have in mind here" may be plausibly translated as "This communication event is open for me now."

"Words can't describe this at all" can be translated as "Every M is completely inadequate relative to questions about this."

"Words cannot exhaust the meaning of the Oedipus myth" becomes "The Oedipus myth is inexhaustibly open."

Whitehead's statement "No verbal formulation adequately expresses a proposition" becomes "Every M is inexhaustibly open for every R at every t ."

Wittgenstein's "Inexactness is sometimes useful" becomes "Some M 's are open and adequate." His "There are different types of exactness" becomes "Some M is completely adequate relative to each of

several different Q's." And his "Thought reaches out to what is the case" becomes "No M is inexhaustibly open."

Urban's "Language is constitutive" might mean "No M is inexhaustibly open" or "Some M's are not inexhaustibly open" or "Some M's are adequate" or "All M's are adequate," or some combination of these.⁸

Wheelwright's "Some symbols have depth meaning" becomes "Some M's are inexhaustibly open." His statement that there are types of adequacy, or that some language is adequate and some inadequate, becomes "Some M's are adequate and some inadequate." I have shown elsewhere how his distinction between scientific and poetic language can be reproduced and accounted for in a way which does not require that poetic language be inexhaustibly open.⁹

Discussion

Someone might object that the foregoing analysis is trivial, on the grounds that the definitions were constructed to facilitate the deduction of consequences chosen in advance. There are several replies to this charge. First, definitions are useful only if constructed to facilitate the deduction of consequences. Second, these particular deductions and consequences ought to appear trivial; for why should there be anything profound about the adequacy of language? In particular, there may be profound differences between art and science, but these differences need not lie in the adequacy of the languages involved. Third and most important, the foregoing analysis and its applications are in many ways far from being psychologically trivial. It is not trivial to distinguish between adequacy and exhaustibility. Furthermore, the analysis of communicative adequacy involves a reversal of our ordinary intuition, in that the descriptive or expressive adequacy of words is taken to be not a relation between message and world or message and sender, but a relation between message and receiver. A similar reversal of intuition is made in the analysis of the adequacy of a message to exhaust a meaning; according to the above explication the inadequacy lies not in M but in the communication analysis. And finally, particular applications of the foregoing analysis will not be trivial in cases where the receiver is motivated to construct an interesting Q and has difficulty deducing A.

Again, someone might object that the foregoing analysis is too artificial, that it is not relevant to real situations or that it falsifies

them. In reply it can be said, first, that the communication situation has been schematized here generally enough to subsume the ordinary language situation, science, and literary criticism. There is no commitment to the existence of explicit semiotic rules, or to any particular choice of D or K. D is not even required to have a rule that from a contradiction anything follows. There is no commitment to the view that art is communicative, for the art-work can be regarded simply as an artifact which evokes a response in the receiver.

It might also be objected that the measure of adequacy is misleading in those cases where its value is less than one, in that the denominator will be too large, because it contains the weights of unanswered questions together with their correlates. It would be easy enough to remove a certain amount of this duplication if any good purpose would be served thereby; but a reason for leaving the duplication in is that this allows for an artistic M which yields both a yes and a no answer to the same question.

Or it might be objected that consequence (1) above is false, in that a person might not know the content of the D he is using and might wish to be told whether some sentence is logically true or not. The reply to this is that if R needs to know whether M is logically true, then M is not part of the D available to R, and the receipt of M is therefore informative to R.¹⁰ After M has been received, of course, it may be accepted by R as part of D or K. Once it has been so accepted it ceases to be informative; it is informative only so long as it is kept separate from D and K, that is, only so long as it is being tested and does not belong to the testing apparatus.

There is a problem regarding Q. One can always choose Q so as to make M either completely adequate or completely inadequate or partly adequate and partly inadequate. Shouldn't we therefore impose some restriction on Q or give criteria for goodness of Q? The lack of a restriction on Q does not affect openness or inexhaustibility, because anyone who wishes to prove openness or inexhaustibility must find good Q's and it is irrelevant whether there are bad Q's. For the case of applying the adequacy measure, however, it would be desirable to have a criterion for goodness of Q. But how could such a criterion be obtained? If we knew how to ask good questions, would we not thereby know the future history of science? We might reduce the problem somewhat by using a device of Putnam:¹¹ We could say that Q is good relative to P to the degree that the answers to the questions in Q are "about" P. Use of this device would still leave us with problems

as serious as the problem the device was supposed to solve, namely, the problems of criteria for goodness of P and goodness of the semantic basis. It seems best at present to leave Q unrestricted, taking advantage of the fact that in actual practice scientists and literary critics and men of business are motivated by various factors to maximize both the richness and relevance of Q and the adequacy of M.

Finally, it might be asked whether the above analysis really exhausts the meaning of 'communicative adequacy', or whether there aren't perhaps other dimensions of adequacy, such as the adequacy of language to formulate the questions in Q. The reply to this is: If you tell me what sort of communication situation you have in mind, the kind of interest that the receiver has, what kind of logic he uses, and so on, then I shall try to develop a logic of questions, answers, rules, and so on, that will satisfy your requirements and will fit within the framework of the foregoing analysis. And you will in any case recognize my answer as adequate only by calculating its ability to answer your questions by your rules in light of your knowledge.

Perhaps the most important contribution of this approach is to show that a message does not provide its own "depths"; it is the receiver who keeps the message open. And perhaps, ironically, it will turn out that the only way we can safely speak of depth language and inexhaustible meaning is by using a quantitative concept.

Testing the Theory

The definitions and theory-sketch given above are parts of a theory of the dynamics of communication which the author is currently developing. When formalized, the theory will constitute an articulation of "rational" communicative behavior. As such, it will be analogous to the theory of rational economic behavior. Like the economic theory, furthermore, the communication theory will be testable in a certain complicated sense, and ought to be tested if its area of practical relevance and actual use is to be discovered.

In testing the theory, however, the following consideration must be kept in mind. If some individuals in an interview or experiment do not behave as the theory says they should, one might plausibly reply that those individuals are not acting rationally and the theory need not be revised because of the way they behave.¹² (On the other hand, of course, a subject who behaves as the theory says he should might be doing so because he is sophisticated, knows the theory, and

“enters the spirit of the game” even though he believes the theory is defective and normally behaves in a different manner.) Therefore, if some subjects do not behave as the theory directs, the theorist has at least the following choices: He can revise the theory until it conforms to the behavior of naive subjects, or experiment further to discover what persons (if any) do behave in accordance with the theory, or experiment primarily on subjects who have been trained to understand and use a variety of communication theories including the present one. One of the final steps will be the revision of some details, either in the statement of the theory or in the statement of the scope of the theory, but the important point here is that such revision should come only after a comprehensive program of testing has been carried through.

The following are intended to be suggestive of the types of test appropriate for the analysis given in this paper:

(1) Naive subjects could be interviewed concerning their conceptions of the adequacy of descriptions and messages. It is highly probable that the result of such interviews will be the conclusion that the present analysis of communication (in terms of D, K, Q, and W) does not constitute a *map* of what most people have consciously in mind when they receive messages, but such a conclusion does not preclude the possibility that this analysis is nevertheless a *model* of how people behave. Cf. the economic theory; a person might behave as if he had a utility function, even though he had never heard of utility functions.

(2) Each of several naive subjects could be interviewed to determine a topic which is interesting to him, but simple enough to experiment with, and then examined by interview and questionnaire to determine his K and D regarding this topic, and whether his interest in this topic is exhausted by some Q. The subject could then be asked to evaluate the adequacy of various M's, some of which are contained already in K & D, some of which are the negations of items in K & D and some of which answer, respectively, none, some, and all of the questions in Q.

(3) Several subjects could be presented with the same experience and told to give a complete verbal description of this experience. This should be controlled as follows: One third of the subjects are told that they will receive a prize (a definite sum of money) for a complete description of the experience, but each inaccuracy in the description will result in a deduction from the prize. Another third are promised the prize without penalty for inaccuracy, and the other

third are promised neither reward nor penalty. The experience presented to the subjects must be rather simple; it might be an arrangement or sequence of visual and auditory stimuli, such as a sequence of simple pictures or diagrams together with a few simple sounds. Within each of the main groups the subjects are divided into subgroups. One group is told that the experiment is a test of vision, another group is told that it is a test of hearing, another group is told that it is a test of eye-ear coordination, and another group is not told anything about the purpose of the test, but all subjects are told to describe completely what they perceive. Within each of these groups, half the subjects are told that there is no time limit; the other half are told that there is a time limit, but they are not told what the time limit is, and in fact the experimenter does not call "time" until he sees that the subject has completed his description.

The point of such an experiment would be to determine the extent to which "adequacy" of description is influenced by *interest* (here, the possibility of reward, and being told what the test is for), and also to discover the presence of the weighting function W (subjects facing a time limit should presumably write statements in order of importance).

A variation on this experiment is to require the subjects to choose an adequate and accurate description from a selection presented by the experimenter.

Another variation is to compare descriptions given by subjects working alone, with descriptions arrived at through group discussion. The group would be motivated by desire to win a team prize. The point here would be to discover whether "adequacy" has an objective, public meaning which is different from its meaning to individuals *qua* individuals. Another purpose would be to compare public and private W 's.

(4) Weather bureau personnel could be interviewed concerning how much the bureau should pay a private observer for weather observations which could be obtained by that observer, but no one else. Assuming that the observer furnished a standard report regularly and accurately, the question is how much should be paid for a complete report and how much for various component parts of a complete report. This situation could be schematized in terms of several observers each furnishing part of the report. To elucidate the K , D , Q , and W present, the question "why?" should also be asked.

A similar test is to interview survey bureau personnel concerning how much should be paid to an expedition for a complete triangulation

from several mountain peaks, and how much should be paid for component parts of this triangulation.

(5) A successful psychotherapy could be analyzed, as follows: The therapist's diagnosis and prognosis are construed as A, where the patient's words are a completely adequate but redundant M. The therapist is asked to examine the transcript of the patient's words and to state which parts of M are redundant, or at what point the patient had said enough, giving the therapist everything he needed to know.

(6) The history of literary criticism could be examined to discover whether D3, D7, and D11 define interesting concepts.¹³ It does seem intuitively that there are sequences of critical analysis, at least within a single literary school or tradition, such that these analyses when arranged in chronological order constitute a communication analysis in the sense of D3. The q's in later Q's might be answered differently from those in earlier Q's, but all the earlier Q's are taken account of in the later Q's (perhaps this is what it means to say that these pieces of literary criticism belong to a single school or tradition). It would have to be discovered whether the adequacy increases over the sequence, and whether the art-work is still open at the end of the sequence.

In some cases one could interview the author of the last (or latest) item in the sequence, asking him specifically about the openness of the art-work. One might construct with his help a proof of the openness or the inexhaustible openness of the art-work. The point of this would be to discover whether there is in any sense an isomorphism between his concepts of openness and methods of proof and the concepts and methods of this paper. In particular we should determine whether inexhaustibility is conceived to be a quantitative notion.

(7) It has been argued by economists that rational behavior has a survival value, so that, although individuals and groups sometimes act irrationally, most people and groups tend to act rationally in the long run, and therefore the theory of rational behavior should be confirmed by the behavior of aggregates in the long run.¹⁴ Similar considerations would seem to apply to the present theory. One possibility of test is to make a content analysis of winning slogans in political contests. The problems involved in such a test seem quite formidable.

Another test of the same type is to compare eyewitness accounts of some event in such a way as to arrive at a group account, and then compare this group (public) account with the account as presented

in "successful" newspapers. Adequacy might also be shown to be a function of the distance between where the event occurred and where the newspaper's readers live.

Conclusion

If it is true that the occurrence of inexhaustibly open statements does not serve to distinguish non-scientific from scientific language, it would be useful anyway to have a criterion by which these two types of language could be distinguished, and if such a criterion were found, it would thereby indirectly confirm the present analysis. Of course it might turn out that the distinction between these types of language is one of statistical distribution of inexhaustibly open statements. Meanwhile, however, logical empiricists are looking for such a criterion by examining the scientific class *K* of assertions, the finite list *Q* of questions and the logical system *D*; philosophers of the analytic school are exploring the varieties of *D* and *Q*; and social scientists are studying the variations in *W*, the weight function by which one assigns to each question a positive number. Whether the results of these investigations can be integrated to yield a satisfactory criterion remains to be seen.

Is it worthwhile to perform tests such as those indicated above? It is worthwhile to the extent that it is worth discovering what people are committed to accepting consequences (1)–(9) (page xxx) of this analysis.

NOTES

- ¹ This is an expansion of a paper read at Pomona College, March 5, 1957.
- ² Alfred North Whitehead, *Process and Reality*. New York 1929; and Ludwig Wittgenstein, *Philosophical Investigations*. New York 1953 (especially sections 69–120). For a dualistic view that one type of language or "semantic strategy" is adequate while another type involves "depth meaning", see Philip Wheelwright, *The Burning Fountain*. Bloomington 1954.
- ³ Wittgenstein, *ibid.*, and Alice Ambrose, "The Problem of Linguistic Inadequacy". Max Black, ed., *Philosophical Analysis*. Ithaca 1950.
- ⁴ Note that the problem of syntactical adequacy, which has been solved by C. E. Shannon in his well-known mathematical theory of communication, is irrelevant here.
- ⁵ For a more detailed proposal on this point, see my "Science and the Rhetorical Aspect of Communication." *Methodos*. Vol. IX, 1957, p. 113. One important difference is that here it is required that the values of *W* be positive.

- ⁶ Concerning the independence of these value assignments, see C. I. Hovland, "A 'Communication Analysis' of Concept Learning." *The Psychological Review*. Vol. LIX, 1952, p. 461. The justifications for requiring that Q include both positive and negative forms of each question are many and complicated. One consideration concerns the values of the adequacy measure given in D4, below. I shall present other considerations in a forthcoming paper.
- ⁷ That is, if K already contains the thing asserted by M, it does so on grounds other than R's knowing that it is asserted by M.
- ⁸ Wilbur M. Urban, "Whitehead's Philosophy of Language and Its Relation to His Metaphysics." Paul Arthur Schilpp, ed., *The Philosophy of Alfred North Whitehead*. New York 1951.
- ⁹ "Explication of 'Depth', 'Level', and 'Unity'," *The Journal of Philosophy*. Vol. LV, 1958, p. 781.
- ¹⁰ It should be clear from this, and from the way the foregoing definitions are formulated, that this explication belongs to pragmatics or empirical semantics, rather than to pure syntax or pure semantics.
- ¹¹ Hilary Putnam, "Formalization of the Concept 'About'," *Philosophy of Science*. Vol. XXV, 1958, p. 125.
- ¹² Cf. the discussion of "irrational" behaviour of people working in teams in Bavelas' experiments on communication networks. Heinz von Foerster, ed., *Cybernetics* (Transactions of the Eighth Conference, March 15-16, 1951, New York). New York 1952.
- ¹³ For an example of a piece of historical analysis used to confirm a theory of "rational" naval tactics, see the discussion of Trafalgar in Frederick W. Lanchester, "Mathematics in Warfare", in James R. Newman, ed., *The World of Mathematics*. New York 1956.
- ¹⁴ Kenneth E. Boulding, *The Skills of the Economist*. Cleveland 1958, p. 60.

ACTIONS AND WAYS OF FAILING

by

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One important class of actions concerns tasks. Questions may be raised about whether we have succeeded or failed to do what we were trying to do. Not all the things we call actions are open to considerations of such success or failure. And questions of succeeding or failing are not raised solely about what we may have been trying to do. The paper attempts to classify various ways in which one may fail in an action; the array that results is explored in terms of evaluations rendered, responsibility, character, deliberate decision, and the like. The analysis of failing in what one has tried to do is made central. To be said to be trying to do something, one must have done the requisite things that constitute trying to do something and not to have succeeded yet in doing what one may have tried to do. "Trying to succeed" in doing something is a redundant expression except for special cases; and though one may decide to try to do something, one cannot decide to succeed in this. One may actually succeed in doing what one has tried to do but one cannot deliberately succeed. Further distinctions are made between trying to do something and wishing to do something and believing that one is trying to do something; also, between failing in what one has tried to do, failing to try, failing to do something without failing in what one has tried to do, and failing to do something in respects in which one could never be said to have tried to do it.

I should like to propose a theory of human action. But I cannot, not having sorted out satisfactorily all of the things one would call an action. So I shall rather attempt to arrange some preliminary distinctions any suitable theory would have to take account of.

I think it is central to our way of speaking about actions to refer to such things as *working at a task*, *doing one's job*, *trying to accomplish something*, *making something*. And the feature of all of these that promises to be useful to analyze is this: we may fail or succeed in our effort. I am not saying that everything we should call an action is open to success or failure; I should not like to foreclose on the versatility of our language. But certainly a very large array of locutions that bear on what we should call actions bear on considerations of success and

failure. On the other hand, not all situations of failing and succeeding bear directly on actions. It is useful to notice that, in baseball for instance, one may say a certain batter, now on first base, "has failed to score." referring not to any action on his part, but rather to the fact that his team was retired without batting him in. But should we say, "He failed to get a hit," or "He struck out," we should be talking about the batter's having failed in an action of his own.

I should like to classify various ways of failing in one's action. But I find that failing applies also to things other than actions and that certain distinctions regarding actions are more easily noticed in comparison with these other things. I think we should not find it unusual to speak of "failing in one's task," "failing to act," "failing to take action," "failing to complete his sentence or toss," "failing to walk," "his failing heart," "a failing grade," "a drama's failing at the box-office," "his failing to write a great novel" and the like. What distinctions can we draw out of this array?

One may of course fail to act. A child is playing in the middle of the street; a car bears down on it; we see the danger, but are frozen with fear—we fail to act. An unarmed airplane is shot down over neutral territory by a hostile nation; our government does not lodge a protest—we fail to act. Notice that in the first instance we do not deliberately fail to act, but in the second we very likely do. So "failing to act" appears to be neutral on this point. "Failing to take action" suggests a refinement in terms of authority, official powers, legal rights, formalities. We should probably not use this expression for the first instance, but probably should use it for the second. Otherwise, the two locutions seem to be employed in much the same sort of circumstance. "Failing to act" applies only to special crises, unexpected events, sudden challenges, responsibilities and the like. One cannot be said to fail to act merely because one is idle. Circumstances are taken to require an effort of a certain kind or at least an effort is normally expected or even thought to be particularly desirable in some respect. Someone beats your son in your presence; you "do nothing," that is, refrain from striking him or protecting your son or calling the police or some such thing. You have "failed to act" or "failed to take action." There is in short a presumption of some sort that one ought, under the circumstances, to engage in an action of a certain suitable kind, though precisely what particular action is suitable is not necessarily indicated. One might take one's son and run, one might turn to fight, one might call the police; for any of these

we should probably not say, "He failed to act." "Taking action" and "failing to act" bear then on meeting in some relevant way the challenge of present circumstances. So "failing to act" means at least not being engaged in an action that would be deemed relevant and that is somehow demanded, whatever other action one may be engaged in. It is for this reason that one's failing to act, in a capacity in which one is required to act (consider a police captain on the occasion of being offered a bribe), is open to censure of various sorts — moral, legal, official, prudential, "Failing to act" looks more toward the criticism of character, intelligence, perception, responsibility than to providing an account of human action, because one may fail to act in a way that is not deliberate (and still be open to censure or criticism, as in having rather stupidly overlooked a situation requiring one's action) and because the emphasis is upon not actually being engaged in a suitable action, whether one is engaged in another action or not. "Failing to act" is used elliptically for "failing to take proper action," either in the strong sense of having acted improperly or in the weak sense of having failed to act in any way that might be presumed to be relevant to the requirements of the situation.

We also speak of "an act of piety", "an heroic act," "a terrible thing to do," "a monstrous deed." Characteristically, we have before us a completed action singled out for our notice, not however with a view to assessing its success or failure. Considerations of success and failure do not arise here; they can only be presupposed where relevant; we are concerned to evaluate some completed action in a way that signifies something of character and responsibility and temperament and the like. So speaking of acts of this or that sort or of one's failing to take action presupposes our way of speaking of particular actions, but is not yet addressed to actions as undertakings.

One may fail to complete an action. A thief has set out to rob a bank. He has made all his preparations, approached the bank itself, and begun to make an entry. But certain chance occurrences have impeded his progress. The traffic signals slowed him up longer than he'd anticipated, a police officer had mistaken him for an associate and greeted him, the handle of the driver's door suddenly had begun to malfunction. Being superstitious, our thief "changes his mind," as we say: he decides not to pursue the robbery. He has failed to complete the robbery, he has failed to complete an action. A suitor who has gradually mustered the courage to ask for some lady's hand finds his affection altered, decides not to propose. He has failed to complete

the proposal, he has failed to complete an action. But the failure in cases of this sort is a little odd. It is not as if someone had tried to do something and failed in his effort; it is simply that one has decided not to attempt to complete what he has undertaken. The thief had been trying to rob a bank; he has not failed in what he was trying to do, he has decided not to try further. He has not, for instance, been apprehended in his effort by the police — he has not failed in this way, he has merely decided not to attempt to achieve the robbery. It is very important to notice that *he couldn't decide to succeed* in the robbery, he could only decide to attempt the robbery. Knowing *what he has attempted to do*, we and he might judge whether *what he has done* constitutes success in his particular action or not. Had the thief made off with the bonds and cash without detection, we should say he succeeded in committing a robbery; had he overlooked a fortune in the bank vault and come away with only several small bags of coins, we should probably say he succeeded in robbing the bank, though the robbery itself was not a success (that is, he was successful in his action insofar as he was attempting to rob a bank, but the robbery once completed is itself judged to be insignificant with respect to the sums gained and the efforts expended).

The marriage proposal seems rather different and is therefore instructive to analyze. Because it is difficult to see how one could fail to make a proposal if he tried to make one. The action we call proposing seems to be merely saying, for example, to some lady: "Elizabeth, will you marry me?" And if one were trying to say this, how could he fail? The answer is, ordinarily one would not be trying to propose, one would not be setting a proposal as a task. Consider an absurd case. Our suitor is bound and gagged and he is trying to get the gag out of his mouth in order to utter the words to his lady friend, who is standing by helplessly, "Elizabeth, will you marry me?" He can surely fail to propose. Or else, he is merely trying to utter these words standing in Elizabeth's presence, forms them with his lips, but is amazed to find that no sound issues from his throat. His mouth clicks open and shut, he gestures rather wildly, but he cannot speak; he has failed to propose though he has tried to — he has not made a proposal of marriage. Or else, he utters the words so softly that Elizabeth does not hear them — he has failed to propose, though here he may not have *tried* to propose. So he may fail for all sorts of reasons, some of which at least may not involve any responsibility on his part and some of which may not bear on failing in a task.

Let us review our findings here. One may attempt to rob a bank and he may succeed or fail in what he is attempting to do. If he decides not to pursue the robbery further, which he has already begun to undertake, we cannot say he has failed in what he is attempting to do, simply because he is no longer attempting to do what he had been attempting to do. The question of failing or succeeding supposes that *we have done* the requisite things that constitute *trying to do something*; thereupon, it becomes relevant to ask whether *what we have done* actually constitutes doing *what we had been trying to do*. The same occurrence then is viewed prospectively in terms of what we are trying to accomplish and retrospectively in terms of what we have accomplished or failed to accomplish. Success or failure in doing something is, in a sense, out of our hands, though it is we who in fact, by our efforts, do succeed or fail. Success or failure has to do with judging whether what we have done achieves what it is we were trying to do. "Trying to succeed" is a redundant expression, in the sense that if we are trying to do something we are necessarily trying to accomplish something, succeed in some project; we could not be trying to do something and trying to fail in what we are trying to do. Otherwise, "trying to succeed" involves a special use of "succeed", for example, trying to build a large medical practice, trying to get enrolled at the country club, trying to get an executive post. *We could not fail in our trying to do something, though we might fail to try to do something*. "Failing to try" means not trying. But insofar as we are trying to do something, we could not fail in our mere trying. It is not that we are always successful in trying to do something, insofar as we are merely trying: it is rather that it makes no sense to say someone has failed or succeeded in his mere trying. The question of success or failure arises only with regard to an action, say, a robbery, and not with regard to trying to perform an action. In short, we cannot be said to fail or succeed in trying to do something, and the question of failing or succeeding in an action supposes that we have tried to perform an action of a certain kind. If we have not tried, there is no point in saying we have failed or succeeded in an action; trying to do something is the condition under which it becomes relevant to ask whether one has succeeded or failed — since he must succeed or fail in what he was trying to do. He can only fail in *what* he was trying to do; he cannot meaningfully be said to fail *in* his trying to do something. We can of course say, "He was not really trying," or "He was not trying hard", by which I think we mean to qualify the way in which someone was trying to do some-

thing; else we may have in mind that, though someone is doing certain of the things involved in trying to do something, he is apparently not concerned with succeeding and hence is not really trying to do what he appears to be trying to do.

We may, as I have already suggested, fail in an action for any number of different kinds of reasons. Our thief may be apprehended in the act of robbing the bank. It may be that in attempting the robbery, he inadvertently dropped his gun and attracted a policeman on his beat. It may be that the officer was inordinately conscientious about making his rounds, perhaps even had a hunch the bank was going to be robbed that very evening; and so, though our thief performed his work perfectly, the policeman spotted him, peering through one of the windows of the bank. It may be that his explosives were damp and failed to ignite. It may be that he did after all manage to open the vault only to find it empty. The important thing is that he did a suitable number of things that entitle us to say he was trying to rob the bank; and having tried, he failed in his attempt — whether through carelessness, lack of skill, accident, bad luck, or the impossibility of the task under the circumstances does not matter. But there are some qualifications that must be entered here.

Let us suppose you find me walking down the road. You ask me what I am doing. I say, "I'm walking to Los Angeles, I'm trying to walk to Los Angeles." You observe that I am walking in the wrong direction and instruct me how to get back to the Los Angeles road. I thank you and follow your instruction. You have a *prima facie* reason for supposing that I was genuinely trying to walk to Los Angeles even though I had been walking in the wrong direction. Suppose however I insist that I am going to Los Angeles my way, which is impossible and plainly known to be impossible to me; you have a *prima facie* reason for believing I was not trying to walk to Los Angeles whatever other purpose I may have had in mind. Suppose however, I made it apparent that I thought you were deliberately deceiving me and, being stubborn, insisted on going on without making further inquiries; you might then have some reason for believing that I was indeed trying to walk to Los Angeles, but that I would inevitably fail. Or suppose I said I was trying to walk to the moon. Even if you thought me that naive and ill-informed to believe such a thing possible, you would not be willing to say that I was really trying to walk to the moon. I might *wish* to walk to the moon; I might even sincerely say I was trying to walk to the moon. But I could not even make the attempt, *I could not*

do anything remotely relevant to walking to the moon. There simply are no things to be done that would entitle us to suppose someone to be trying to walk to the moon, regardless of what he might say or how sincerely he might believe what he said. One might then *believe he was trying* to walk to the moon though nothing he might do would ever suffice for us to say he was *actually trying* to walk to the moon. Still, we should want to say our thief was actually trying to rob the bank, though the vault's being empty made it impossible for him to succeed. It is important to notice that it is not necessary that one be able, under the circumstances, to succeed in the action he undertakes; it is only necessary that he believe he could achieve some projected goal by means of what he is now doing and that he actually do the sort of thing that would entitle us to suppose he was trying to achieve that goal. The alchemists of old might well have been trying to change base metal into gold; they were certainly doing what, at that time, was tantamount to trying, however impossible the feat may now be known to be. So one must at least be behaving in a way suitable to an attempt to do something of a specifiable sort; and one must also believe that what he is doing could conceivably lead him to be successful in what he has projected as his goal — for instance, he might have a plan he is following. But if he does not do the requisite things, he cannot be said to be trying to do some further particular thing; and if he does not believe that what he is doing could conceivably lead to success, he could not be trying to do what he believes he would fail in.

So an action must be a complex system, since one must be supposed to be trying to do something, but not yet doing it; and while engaged in trying to do something, to be capable of succeeding or failing. There must be a series of things that we may do, some of which, under appropriate circumstances, constitute trying to do something and some of which, succeeding or failing actually to do something. There is a sense then in which an action is completed even in failure. This was of course the reason we had to seek beyond those instances in which one failed to complete an action in the weak sense in which one merely stopped trying to achieve his goal. If, in a contest, I shoot an arrow at a target, I am trying to hit a bull's eye; and regardless of my score, the action is completed when the arrow finally lands. Essentially the same sort of consideration applies to every action of the kind we are speaking about. The action of robbing the bank begins with one's trying to rob the bank and ends with, say, the hour the night

watchman makes further effort impossible. The suitor's proposing begins with his trying to propose — for instance, his greeting Elizabeth with flowers and a warm smile — and ends when he closes her door behind him at the end of the evening. The limits of an action are debatable. One may ordinarily say, "He hasn't succeeded yet, but he's still trying." An archer thus might be trying to hit a bull's eye; he might also have been trying to hit a bull's eye on his first arrow. He would have failed decisively in the second action (in failing to hit a bull's eye), but we may also conceive his action more generously and wait therefore for the contest to end.

It may also be noticed that the things one does which constitute trying to do something may, in their own right, be viewed as actions in which one may fail or succeed. There is no qualitative difference, as far as behavior is concerned, between the kinds of movements that enter into what we are trying to do and what we in fact fail or succeed in doing; the difference lies only in the status we assign, the perspective in which we view, the components of any action. I may be trying to hit a bull's eye, but I may also be trying to arch a very heavy bow; it may also be of course that I should have to arch that bow in a certain contest in which I was trying to hit a bull's eye, though, in such a case, it would be very odd to say I was trying to hit a bull's eye if arching the bow was itself in such doubt that one should want to say I might fail in the effort. Only after I have then succeeded in arching the bow, considered now as a distinct action, could I be said to be undertaking to try to hit a bull's eye, this being something requisite to trying to hit a bull's eye yet still short of the achievement. It is in circumstances of this sort that one might say, "He *wants* to hit a bull's eye, but we have yet to see whether he *can make a try* at it." The question of failing or succeeding in an action cannot legitimately arise until one has in fact done whatever sufficiently constitutes trying to perform that action. The conditions of an actual try are as variable as those of an action; all we need note here is the logical dependence of the question of success or failure on granting that one is making a try. But if what constitutes trying to do something is in fact itself viewed as an action in which we may yet fail, the question of succeeding in the ulterior action cannot yet arise.

Otherwise, we have an ellipsis of some sort. For example, someone tells me he tried to enter Communist China, but didn't succeed. I ask him how he failed, and his answer is he didn't get State Department clearance to leave the country. He was trying to get his passport

papers cleared for travel to China and regarded success in this as tantamount to success in entering China. But should we insist that there were other things he would have had to try to do in order to be said to be trying to enter China, besides trying to get his passport papers cleared, his remarks would be taken to be misleading. So "trying to enter Communist China" may be a shorthand expression for a series of undertakings for which merely trying to get one's passport papers cleared for travel in China is not an adequate surrogate. Still, not all the things we do in trying to enter Communist China are things we have tried to do. That is, we do many things which we have not, in any significant sense, succeeded in doing. Else, otherwise, "success" is an altogether redundant expression applying to whatever we do; though we must notice that "failure" can hardly be said to apply to whatever conceivable behavior lies outside the range of what we actually have done. In short, succeeding and failing apply to those things we *deliberately try to do*. Not deliberately do, but deliberately try to do. I might deliberately write a letter to the State Department requesting information about passports, though it might be puzzling to ask in what sense I might succeed or fail. We should have to conjure up strenuous cases in which I had set myself a task in which I might succeed or fail (as we have already tried to do in speaking of a suitor's proposal of marriage). Just so, we might ask, "What did he do?" The answer: "He wrote to the State Department requesting information about passports." The question, "What did he try to do?" might not relevantly arise, though we might ask, "What *is* he trying to do?" That is, what he has done could be construed as part of an effort to try to do something else; and this returns us to the distinctions already drawn. Also, if we are prepared to call whatever we have done an *action*, it is clear that we are describing here only that sub-set of actions that concerns tasks.

We have already touched on situations in which we could not conceivably be trying to do what we believe we are trying to do. I could not, thus, actually try to walk to the moon. I could not, because the most generous extension of the term would not allow us to single out a set of things I might do that would suffice for it to be said I am indeed trying to walk to the moon. There is nothing I could undertake that would enable us to distinguish between my wishing to walk to the moon and my trying to walk to the moon. But there are some important and related cases that seem to require an adjustment. Could I try to write a beautiful poem? Could I try to do a memorable deed?

Could I try to make a great discovery? There is something obviously puzzling about these questions. Because we are prepared to say that someone, who is writing poetry, has failed to write beautiful poetry; that someone, engaged in some inquiry, has failed to make a great discovery; that someone, called upon to make decisions, to act, in some position of significant responsibility, has failed to do a memorable deed. But we are not certain we should say that any of these people tried to do what they are said to have failed to do. Imagine also that I am a shepherd musing beside you on the hilltop: "I shall lead an army some day and conquer Asia for the homeland!" You take notice of my *wish* and, toward the end of my life, observe that I have *failed*. But I have failed *in my wish* (to fulfil my wish) and not *in what I have tried to do*; because I never once really tried to gain command of an army. Is it perhaps the same pattern that we find in these other matters? We may *presume* that a poet *wishes* to write beautiful poetry in that he deliberately tries to write poetry; and hence, since he has already made the significant effort of trying to write poetry (which is much more than merely wishing to lead an army to victory), it is not altogether impertinent to remark that he failed to write beautiful poetry. Since it is pertinent to make judgments of this kind when one has merely wished that something were so, *a fortiori* it is pertinent when one has tried to do something sufficiently closely related to what it is he is supposed to have wished to do.

As a matter of fact, it is not really necessary to suppose that a poet who is actually writing poetry wishes to write great poetry; it is sufficient that we are interested in judging whether someone's poetry is great or not. To say, "He failed to write great poetry" might mean merely that the poetry he did write was not great; it may be known in addition that he wished or hoped he would write great poetry and this may make our judgment of his poetry all the more poignant. So we might very well fail to do what we did not even wish to do. *A fortiori*, we might fail to do what we did not try to do. It is the latter kind of case, however, that is the more interesting, because it forces us to distinguish between failing to do what we did not try to do and failing to do what we tried to do. And a great many value judgments are occupied with the first kind of failure. "He failed to say anything interesting" — said of a lecturer; "He failed to make a name for himself" — said of a writer; "He failed to produce any great sculpture" — said of a sculptor; "He failed to impress us" — said of a new acquaintance. These need by no means be superfluous

expressions, though they will admittedly be dependent on such evaluations as, "His sculpture is not great sculpture," "His talk was not interesting," "He is not an impressive person."

Now, I think someone may try to impress us with his poetry, but I do not see how anyone could try to write impressive poetry. I see how someone might try to gain the reputation of being a great sculptor, but I do not see how anyone could try to make great sculpture. In short, I believe the second of each of these pairs of would-be enterprises is rather like trying to walk to the moon. We are not prepared to entertain any effort as being relevant *to the effort* to produce great poetry. Someone might produce great poetry, but he could not *try* to produce such poetry (he could only try to produce poetry, even poetry like the poetry most recently characterized as great by the critics, and he may try to improve his poetry). What is of importance to notice here is that such terms as "impressive," "great," "beautiful" refer to products or actions judged, upon completion, by criteria not, in principle, available to the original agent or producer as rules or recipes of action or production. And this is so, either because the judgment, insofar as it refers to an action deliberately undertaken, judges gifts or capacities — with respect to the having of which the question of effort is quite irrelevant; or because the critics themselves cannot formulate the sufficient conditions of beauty, impressiveness, greatness, profundity, and the like and cannot themselves say antecedently which among proposed undertakings would qualify (though they characteristically disqualify imitations, which require plans of action). If one could know what would count as a great work of art, as one may know what would count as a good deed, he could try to make one and even fail or succeed in what he tried to accomplish. As it is, he may only fail or succeed to produce a great work of art without being able to try to produce one. I suggest that such "properties" of a work of art as its greatness or beauty, or similar properties of actions and products in other contexts, are *supervenient* properties: that is, one cannot try to produce them though one may produce them (or produce other properties in virtue of which they may be assigned to some object or action). And this seemingly roundabout analysis permits us to see, incidentally, a profound difference between the usual use of moral predicates like "good" or "right" and esthetic predicates or general cultural predicates like "beautiful" or "great" or "profound" or "impressive." One can try to do a good deed, but one cannot try to say something profound.

So in sorting out a large variety of ways of failing that bear on actions, we have been led to see that failing in what one has tried to do appears to be basic. Failing to act or to take action, failing to complete an action, failing to try hard, failing really to try, failing in what one wishes (without yet trying), the failure of what one does or makes as distinct from failing in what one has tried to do — all of these ways of failing trade on failing in what one tries to do. We are led to see also that actions involving trying to do something, actions in which a task of some sort is set in which we may fail or succeed, cannot form more than a sub-class of actions, of the things that we are characteristically said to do. Because even in trying to do something, an action which must be complex in a manner already indicated, we must in fact *do* something that constitutes *trying to do* what we have set as our as yet unaccomplished task. Trying to do something is doing something of course, namely, doing what we call “trying to do something.” If I am trying to catch a fish, I am doing something — for example, casting a fishing line with bait attached. I must be doing something of this sort in order not yet to have done what I was trying to do, namely, succeed in catching a fish.

What I am now doing is the activity I am now engaged in. It may be useful to observe that “doing” usually, but not always, refers to activities that may plausibly be construed as part of what one may be trying to do. If I am casting a fishing line with bait attached, I am fishing — that is what I am doing; and what I am doing is ordinarily construed as trying to do something, namely, catch some fish. I may also be engaged in baiting a hook, in chatting with another fisherman, in studying the technique of another fisherman, and so on. And all of these things may or may not be part of trying to catch some fish, but all of them are things I may be doing. There may not, in short, be any plausible task I may be said to have set myself in doing these things. Also, it is not altogether curious to answer to the question, “What’s he doing?,” “He’s sleeping; he wanted to talk to you, but just couldn’t keep awake.” And here we see that, at least sometimes, we use “doing” in a broader way than “activity”. The difference seems to depend here on taking an activity to be equivalent to one’s deliberately doing something. However, even here, we must notice that one speaks of the heart’s activity, which perhaps is at least able to be treated analogously with human activities, that is, in a way plausibly connected with a would-be task (for example, maintaining a certain metabolic routine). And therefore, we speak also of the heart’s failing.

Trying to do something is in fact doing something — this is important to emphasize. There must be suitable body movements, exertions, to speak of someone's trying to do something. If, at a cocktail party, I introduce you to a man I say is trying to send a manned rocket to the moon, you are to understand that he has already done certain things that warrant my speaking of him this way. And if it turns out that he has only wistfully expressed an interest in such a project, or even that I was merely guessing from his formidable gaze, I should be mistaken in saying what I did. So trying to do something is doing something. And failing to do what one has tried to do is simply not doing whatever the fulfilment of the task set would require; but still doing enough to be said to have failed in one's effort.

Beyond this, the questions branch toward considerations of responsibility for one's action and evaluation of what one has done or made.

THE IRRESPONSIBILITY OF AMERICAN SOCIAL SCIENTISTS

by

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The arguments contained in books criticizing American social scientists by C. Wright Mills (*The Sociological Imagination*) and Bernard Crick (*The Science of American Politics*) are discussed, compared and criticized. It is argued that Mills' criteria of evaluation and constructive alternatives to the tendencies he criticizes are immeasurably sounder than those found in Crick's book. An effort to supplement Mills' argument by providing a more explicit statement of its moral underpinnings is made. Finally, it is argued that though both critiques have serious empirical flaws, the authors have undeniably demonstrated that important tendencies for irresponsible thought and action do exist within the American social sciences.

Bernard Crick and C. Wright Mills have both written books alleging that those who dominate American social science are misguided and profoundly irresponsible. In his book *The American Science of Politics*¹ Crick focusses on the political scientists. In *The Sociological Imagination*,² Mills argues more generally, but his special targets are the sociologists.

Both men have written with unaccustomed nerve and boldness in these academic dog days of excessive caution and excessive jargon. The reasons both give for their dim view of American social science are often the same or similar. But there are important disagreements as well.

I shall discuss the arguments these men develop, trying both to point out these similarities and differences and to assess their respective efforts. I shall argue that, though both books are valuable as diagnoses of present ills, Mills proposes a far sounder constructive alternative than does Crick. In fact I shall argue that Crick's basic presuppositions are thoroughly unsound. I shall then supplement Mills' analysis with an account of the basic commitments which I

believe underly his general critique of American social science — an account which I hope will be more explicit and more coherent than the one he provides. Finally, I shall briefly discuss the factual adequacy of their accounts of the current state of the American social sciences.

1.

Both authors' basic concern is with the irresponsibility of the typical social (political) scientist. This irresponsibility is not always, or indeed usually, deliberate. It is more a matter of what is *never* considered than of the things explicitly considered.

More than any other intellectual factor, it is the enormous prestige of physical science and of industrial technology which accounts for the prevailing irresponsibility. On this too the authors agree. Nothing helps explain the character of the typical social investigator's mind, the style of his investigative efforts, the very rhythms of his scientific language, more than this prestige. Most social scientists try to fit their labors into a Procrustean bed of 'truly scientific investigation' in a way which is not only morally irresponsible, but theoretically rigid and irrelevant. This influence is reinforced by a misleading philosophy of the social sciences. According to this philosophy, if an investigation is not based on a *precise* formulation of a problem, which in practice means the use of strictly operational definition and of quantitative measurement, it is not really scientific. This emphasis represents an extreme reaction, not to what Mills calls the classical sociological tradition, but to the literary and metaphysical traditions of social analysis. Whereas those who wrote within the latter tradition seemed to be interested in the moral and theoretical implications of their work to the exclusion of methodological considerations, those who write within the present tradition often seem to select their problems solely on the basis of methodological criteria, to the exclusion of moral or theoretical considerations. Mills calls the group of social scientists who tend to select their problems in this way, *abstracted empiricists*.

The two authors also deplore the way in which American social scientists neglect history. This they agree is one of the most important by-products of the preoccupation with method, and constitutes a great weakness in their whole approach. But in this connection one senses that Crick and Mills actually disagree about the uses to which history ought to be put. Crick seems to view history as a basic source of those intimations which can alone enable us to understand a given society

and which alone points direction to political effort. He does not say so explicitly, but he seems to be taking very much the sort of position which Michael Oakshott develops in a published lecture.³ Mills, on the other hand, regards the fruits of historical investigation "as a great file indispensable to all social science" — as a source of data as well as inspiration for the social theorist.

Their respective views of the uses of history bear on another important area of agreement and disagreement. Both Mills and Crick insist that it is neither desirable nor possible to avoid theorizing. Theoretical considerations are constantly at work guiding the selection of problems and the development of concepts. This is so whether or not the social scientist is prepared to acknowledge the fact. If he is unconscious of the way in which his instinctive theoretical prejudices determine his scientific moves, then he proceeds blindly and, to that extent, in a theoretically irresponsible way. Thus he should aim at explicitness and coherence, in that way recognizing the nature and significance of theory for social scientific investigation. But — and this is where the differences creep in — Crick, in line with his conception of the uses of history, is not as emphatic in his defence of theory as is Mills. One senses that it is the pursuit of historical intimations — the use of traditional perspectives — which is the important thing for Crick. Theory comes into the picture only insofar as it facilitates that pursuit. This comes out more clearly in the positive statement which shall be considered in the next section. Mills, on the other hand, emphasizes theory in a more radical and emphatic way. One senses that for him theory is precisely the indispensable condition of our breaking away from the hold which traditional perspectives exert upon us.

This description of Mills' position needs to be qualified. It is not only anti-theoretical, fact-intoxicated social scientists who depart from the older and sounder classical tradition of social analysis who pervert sociology, but also those whom Mills terms, "the grand theorists". Mills regards Talcott Parsons as the most important and, in a way, prototypical grand theorist. Consequently, he trains all of his critical guns on Parsons' work.

He argues that Parsons misconceives society as consisting essentially of rule-governed activities because he is overpreoccupied with the problem of explaining social order. In so doing he neglects social structure. By "social structure" Mills means, "the combination of institutions classified according to the functions each performs." In neglecting this dimension of social order, the grand theorist not only

fails to explain or anticipate basic kinds of social change, he also neglects the role that power (in a very broad sense which embraces, e.g., manipulation of others) plays in determining human activity. It is only by focussing on social structure that we can explain basic social change, and, in the process, can identify the levers of power required to change those features of the social order which are bad, or thought to be bad. It is irresponsible to neglect precisely those studies and formulations which are useful, sometimes indispensable, to melioration of basic social ills. Thus the political thrust of Parsons' sociological theory is conservative, despite what he or anyone else thinks about its moral implications.

This analysis makes it clear that though Mills believes that theory is important — in large part because it is a weapon in the arsenal of the radical social critic — he does not regard *any* kind of theory as useful. He particularly repudiates those theories which view society as composed essentially of rule-governed activities.⁴ Moreover, he doubts that any very general theory could be useful. He argues that the nation state provides the most satisfactory level of generality for the social theorist on the grounds that, "In our period, . . . , in terms of power, and in many other interesting terms as well, the most inclusive unity of social structure is the nation-state." (135) Excessive generality is connected ideologically with emphasis on rule-governed activity in that it too vitiates the political utility of social theory for anyone interested in changing particular national societies in particular ways.

But, with these qualifications, Mills emphatically stresses that theory is an indispensable requirement of fruitful and responsible social investigation.

2.

Having described the more important points of agreement between Crick and Mills, we can now turn to the basic disagreements. These we can get at most conveniently by considering Crick's positive position.

Crick's basic point of departure is his belief that American political science did historically and does still enshrine the unexamined pre-suppositions of an earlier liberal *weltanschauung*. The main elements of this form of liberalism are optimistic conceptions of man and history, faith in reason and belief that it is the prime mover in human affairs, commitment to a completely secular view of man and the universe, and belief in freedom and a radical form of democracy. According to

Crick these commitments are implicit in everything the American political scientist does — his selection of problems, his conception of the right methods, his moralism and his amorality, his tendency “to delay the task of rationally clarifying the hard from the soft assets of a political heritage.” (239)

He argues against the American science of politics so conceived along two lines. He contends, on the one hand, that these liberal presuppositions lead many into flat contradiction because there is fundamental tension between the belief in reason and the belief in freedom and democracy. But more basic is his critical rejection of the elements of what Crick calls liberalism.

No doubt Crick puts his finger on an open theoretical nerve of traditional liberalism — the tension between reason and liberal democracy. In a brilliantly argued chapter he shows how lack of any critical resolution of this tension leads Harold Lasswell into contradiction. On the one hand Lasswell believes in freedom, democracy, human autonomy, and so on; on the other he believes in the rule of scientific experts, of people who know better than the individual himself what is good for him, and even what he wants, in technocracy as a way of politics. Now while it is true that this is a problem, and that some American liberal political scientists like Lasswell are quite unreflective about it, it is simply not true that all those to whom Crick imputes this incoherence are equally guilty, equally unreflective. In particular this problem was for John Dewey in many ways *the* problem of political philosophy. And in this he was simply following John Stuart Mill whose *Representative Government* was essentially an effort to provide a suitably liberal solution to the problem of incorporating reason in human affairs. It is true that Dewey sometimes naively supposes that there is a close analogy between a democracy of participation and scientific method. But other liberals *within the same tradition* have criticised Dewey on this score, and Dewey is by no means unreflective about this identification. In *The Public and Its Problems* he puts the issue at the center of his analysis and comes out with some much qualified conclusions.

The point is that Dewey and others whom Crick attacks see this as a problem, and Crick does not. Why not? Because basically he has no sympathy for those who would incorporate reason in human affairs as systematically and completely as Dewey recommends. Reflected in his thinking is the pre-eminence of mystery. Like Hans Morgenthau, whom he frequently refers to as an example of the right

sort of American political scientist, Crick believes that, "Scientism (by which Morgenthau means the secular liberal belief in the efficacy of scientific inquiry) has left man enriched in his technical mastery of inanimate nature, but it has left him impoverished in his quest for an answer to the riddle of the universe and of his existence in it."⁵ Both Crick and Morgenthau believe that because we can formulate the problems there *must* be answers; but that the canons of proof and discovery employed by scientists can not help us in finding these answers. Their continued application to problems simply disclose new mysteries — which, however, are only mysterious relative to scientific canons. There are transcendental grounds of explanation. That Crick believes this (though he never argues it) comes out clearly in passages like the following:

The scientific-humanists are dangerously close to making that identification of human worth with right reason and then civic virtue that was the typically pagan element in the politics of Classical Antiquity. Our modern notion of liberty would be inconceivable but for the shattering of this identification by the Christian revolution of the first centuries A.D., a turning of men's thoughts to understand why even (or especially) the poor, the ignorant, the unreasonable, and the unsocial have a transcendent worth (dignity?) that should be loved and can at least be respected. I do not wish to argue that Christianity is the only ground on which this reduction is possible — I do not know, or know how to know, if this is the case. *But I do argue that the grounds of some such an order exist.* (p. 224, my italics.)

There is a certain type of intellectual who might be called the crack-seeker. He is the person who continually scrutinizes the immensely high and strong wall of scientific achievement in order to find cracks into which he proceeds to pour his personal, non-scientific (non-factual) convictions. The wall of science does have its cracks — there are mysteries — but *nothing* follows from this. The existence of mystery does not justify the illusory belief that there must be some transcendental order which provides rational grounds for all mysteries. Once one recognizes this, once one recognizes that the proven patterns of investigation and proof, whether one wants to call them scientific, rational, the canons of intelligence, or whatever, are man's ultimate resource in this attempts to grapple with the problems of his condition, such as it is, then one spends less time deploring the weakness of reason and more time trying to make good use of what is at hand.

All of Crick's other basic criticisms of the liberal ethos that he describes are connected with this scepticism concerning the efficacy of reason, and the repudiation of secularism. His critique of optimistic conceptions of human nature and of history flows from it as well as his general conception and critique of scientism, and his criticism of the kind of participatory democracy which John Dewey advocated.

Turning to C. Wright Mills, we see how misleading are the similarities between the diagnoses of the ills of American social science given by these two men. Though Mills rejects "scientism," he wishes to reinstate in its place a *more* robust faith in reason, a *more* robust belief in the value of democracy and of freedom, a reaffirmation of the value of *secular* resources. While somewhat pessimistic about the prospects of remedy, he insists that reason is man's ultimate resource, and that American social scientists have all too often turned their backs on this resource either explicitly or implicitly. Belief in democracy is for him belief "that those vitally affected by any decision men make have an effective voice in that decision"; a conception which he, unlike Crick, knows is *not* prevalent among contemporary social scientists for whom a bastardized Michels is god, and Schumpeter the first apostle. The following is a characteristic passage in Mills' book, and how different is its spirit from that which pervades Crick's:

Science, it turns out, is not a technical Second Coming. That its techniques and its rationality are given a central place in society does not mean that men live reasonably and without myth and fraud and superstition A high level of bureaucratic rationality and of technology does not mean a high level of either individual or social intelligence. . . . For social, technological, or bureaucratic rationality is not merely a grand summation of the individual will and capacity to reason. The very chance to acquire that will and that capacity seems in fact often to be decreased by it. Rationally organized social arrangements are not necessarily a means of increased freedom — for the individual or for the society. In fact, often they are a means of tyranny and manipulation, *a means of expropriating the very chance to reason*, the very capacity to act as a free man. (169, *my italics*)

I think Mills is generally right, Crick generally wrong about all these basic issues. At the level of description and explanation Crick mistakes what may once have been for what exists today. In point of fact, political scientists tend to repudiate much of the liberal ethos he describes. They do not generally believe in inevitable progress,

they generally repudiate the radical conception of democracy Crick describes, they are generally sceptical of the power of reason in shaping human affairs, and their secularism or lack of it is not particularly evident in their work.

The inadequacy of Crick's account is reflected in the lack of any sociological analysis of the phenomena he purports to describe. In this respect Mills' analysis is immeasurably stronger. For Mills tries to show how the existing social structure, the pattern of power relationships, and the resulting structure of bureaucratic control and influence, help shape the character of American social science (and of American university life generally). Whether or not Mills' account is factually sound is a question to which we shall return.

Though Crick makes hesitant suggestions along these lines, his general account is over-intellectualized. He treats the political scientist's methodological preoccupations as due essentially to bad metaphysics and a wrong appraisal of the nature and achievements of the physical sciences (again echoing Morgenthau's treatment of the same subject). Mills does not waste too much time on the intellectual problems — for he senses their limited relevance. What he aims to show is how convenient — economically, politically, and in many other ways — methodological austerity happens to be in mid-twentieth century America. The potential power of his critique consists in his attempt to show that the highest 'scientific' virtues can and often do mask the deepest forms of moral and political irresponsibility, *and* in explaining *how* this can happen.

In addition to a better account of the phenomena, Mills' argument seems to me to be sounder morally and metaphysically than Crick's. Mills sees, as Crick does not, that those who continually emphasize the weakness of reason, cut the nerve of those efforts which can achieve what reason alone is able to achieve. The same holds for the perennial debate about human progress and for the same reason. Crick's analysis of human progress in fact rests on a systematic confusion. The confusion is between the *inevitability* of progress and the *possibility* of progress through human agency. By criticizing the former, one has not destroyed the latter. That Crick believes that criticism of the one undermines the other comes out in statements such as: "Now, if there is nothing 'inevitable' about progress, there seems only an emotional reason for using the concept." (p. 239) and "(American pragmatism) was willing to criticise almost everything except the pride in technology and the absolute faith in progress which became

called 'scientific method'." (p. 232) Both these statements presuppose that no importance attaches to distinguishing between inevitable and possible progress. But that there is a point to the distinction becomes clear if one accepts the argument given above — that the belief in the possibility of some good (the efficacy of reason, human progress) may be causally related to our efforts to secure that good.

And the same arguments hold for Crick's general pessimism about man. One does not have to believe that man is innately good, nor even to deny that there are instinctive tendencies to aggrandize and destroy in all humans, to believe in the perfectibility of man. For, as Charles Frankel has argued,⁶ the doctrine of the perfectibility of man asserts not that men can become perfect, but that one is rarely if ever entitled to claim that any particular person has reached the limits of his capacity for moral and intellectual development on any particular occasion.

What we have in the respective arguments of Mills and Crick is not so much a confrontation of conflicting doctrines as a confrontation of different tempers. At the risk of oversimplification and misunderstanding let me call them the American and Continental tempers. Mills, in spite of his bold and persistent criticisms of American society has thoroughly American reflexes. Would Crick include him in the group of social scientists he is criticising? I think he must in spite of the fact that Mills implicitly repudiates many of the things on which Crick builds his criticisms. But the argument against the Continental temper, an outlook which often takes the form of a weary and superior cynicism, is partly that it is not and could not be justified by an appeal to the facts, and partly that the temper one has is causally relevant to the achievement of certain values. Like William James' lover (in *The Will to Believe*) it is partly by stressing possibilities, or at least *not* insisting prematurely on impossibilities, that we can achieve certain aims.

Finally, Crick nowhere argues for his rejection of secularism. In a passage quoted earlier Crick expresses his passionate commitment to the principle of human dignity, and he argues that there must be some non-scientific, non-secular grounds on which to base this commitment. But the necessity Crick feels is entirely gratuitous. Even if no scientific, nor any kind of secular proof, can be given, it does not follow that some sort of non-secular proof *must* be possible. This again is the same sort of *non sequitur* one finds in Hans Morgenthau's arguments. There may simply be a kind of existential commit-

ment, or a reasoned argument which, while falling short of proof, gives a kind of secular basis to the commitment. (The sort of reasoning I have in mind can be found in Stuart Hampshire's, *Thought and Action*.)

In another passage, Crick argues:

"... liberty ... is not an end in itself — no more than the liberation or expansion of 'personality', but is the opportunity and the ability to attach value to concepts that are not of themselves directly political, social, or indeed, concerned with the mere business of staying alive." (p. 223)

But the same objections can be made here as were made in connection with the principle of human dignity. I would agree with Crick that liberty is not an end in itself (as many have recently been arguing; notably Sir Isaiah Berlin, in *Two Concepts of Liberty*, John Plamenatz in an article titled, *Equality of Opportunity*⁷ and Henry Aiken in an unpublished paper recently read to the American Conference of Legal and Political Philosophy), but only because I believe that it is both possible and desirable to defend liberty in terms of other and more basic values such as personal development. But I would not pretend that this is much more than a general commitment (which can be defended) to the enterprise of achieving as much theoretical simplicity and coherence as one can in moral theory, as well as to the more basic values, and to their priority, in terms of which liberty is defended. Agreement with Crick on this issue does not point the way to belief in a transcendental order, as he seems to suggest.

3.

I have been hard on Crick's book because I disagree so basically with some of its presuppositions. Yet this is in a way unfair. For the book is essentially a critique and only suggests a constructive alternative by the way. The critical exposure is often subtle, intelligent, and provocative. Even so, for reasons that I go into in the last section (cf. pp. 114—15) as well as the preceding one, even Crick's factual claims about the present state of American political science are suspect. Though he has taped some of the important tendencies within American political science well, and though he has shown how unthinking and irresponsible some of those who carry the dominant tradition are, his case against the American Science of Politics is by no means overwhelming. For he is led by an unsound conception of scientism and an unsound rejection of what he regards as unthinking liberalism to overplay his hand.

The essential difference between Mills and Crick might be put most clearly in the following way. For Crick the key to the alleged irresponsibility of American political scientists is almost wholly intellectual; whereas Mills sees the plight of social scientists as due primarily to the action of large social forces. Their remedies differ in corresponding fashion. Crick tells us that political scientists need more metaphysics, more history, more explicitness — ironically, he seems optimistic about the effect of a purely intellectual appeal. Mills, on the other hand, doubts that the minds of many social scientists will be changed by anything he has written. Undoubtedly, his hope is that he will influence those outside the fields in ways which will exert effective pressure on social scientists — perhaps through a reorganization of university life. But such aspirations are muted in the book. Nevertheless, Mills' book must be viewed as part of his general critique of American civilization.⁸

4.

Despite my general agreement with Mills' position, it seems to me that it can be strengthened by a more explicit and more systematic statement of the general grounds on which it rests. This section is devoted to that statement. It moves from a general account of the nature of responsible human action, to a specific application to social scientists.

All human activities have implications. Either they have causal consequences, or, because they represent a specific use of scarce or strategic resources, they result in the failure to accomplish certain things. For example, because of Britain's historic educational priorities at the start of the second World War, Britain did not have nearly enough scientists. And because scientific manpower was diverted to immediate war needs, Britain's atomic development program lagged badly — a fact that could have been catastrophic had not America entered the war with immensely greater (proportionally) scientific resources. Had Germany built the bomb before Britain, the inevitable tragedy would have been an implication of the things Britons actually had done (e.g. regarding allocation of funds for education) before and during the war. Whether one wants to call the original decisions and consequent actions 'causes' or not is a matter of indifference.

A fully responsible person is one who is aware that his actions have implications, makes a reasonable effort to determine those implications before he acts, and makes his eventual decision concerning that action (or those actions) in a morally serious way.

What constitutes *reasonable effort* cannot be determined in a general way. It depends. It depends upon the specific circumstances of the situation, the importance of projected action, the capacities and resources of the actor.

Moral seriousness consists in trying sincerely to live according to publicly acknowledged and reflectively achieved moral commitments. That is, a person is *not* morally serious if his moral pronouncements are rhetorical baggage which he carries around for use only on holidays from his functional existence. A person is *not* morally serious if his moral decisions are the outcome of impulse, felt need, casual hunch.

Every human being has an obligation to be fully responsible. This widely held commitment illuminates the fact that the term "responsible" is used in at least two ways — one like that described, and one which is involved in *holding someone responsible*. A person who is not responsible in the sense defined, should be held responsible — i.e., should be blamed or punished as circumstances require.

One of the fundamental moral problems of Western civilization is generated by the fact that many (probably most) men do not feel that they do have an obligation to be responsible in the performance of their jobs. Or if they do feel that they have an obligation to be responsible, that obligation seems to them to be limited by the job requirements imposed externally (by the boss, by their professional association, by their trade union, etc.); or they believe with an insensitive naiveté which has to be experienced to be believed that their job activities will automatically be beneficial. They lack any feeling that they have the right and responsibility to impose criteria of significant human activity on what they do in their job. How often and how explicitly does one hear it argued (in private) that one can only be expected to do the prescribed job, and not worry about its significance and implications. Leave the overall view to others; for myself, My Station and its Duties.

The problem has many causes. The concentration of power, bureaucratic sanctions, economic sanctions, the distress of the intellectual and emotional efforts inherent in responsible deliberation, and so on. In a fundamental sense the fact that I have described represents the most radical exclusion of reason from human affairs in modern industrial societies. For nearly everyone is required by economic exigency to take a job, and nearly every job consumes a large portion of a person's life.

Many regard academic and scientific people as having a privileged and highly strategic position in regard to this problem. They envy

the fact that academic disciplines are relatively autonomous, and that, therefore, the academic person has a relatively great opportunity to integrate his functional existence with his personal life in a harmonious, responsible, and satisfying way. But this picture is partly misleading. For example, the typical scientist has very little control over the selection of basic problems for research, and the uses to which his products are put. This fact produced the extremely tortured soul-searching among atomic scientists that C. P. Snow describes in his novel, *The New Men*. Frustration causes many young scientists to abandon their efforts to be responsible; an alternative they find preferable to sacrificing the general satisfactions of their jobs.

Mills' basic thesis is that American social scientists have generally abandoned responsibility without necessity and without adequate defence, and that this abandonment has pernicious implications. He argues that the unexamined scholarly life is so well worth living that many of its practitioners fastidiously avoid self-scrutiny and self-appraisal, and heap contempt on or are amused by those who engage in such soft-headed activities. Their preoccupation with method, with rigorous scientific standards, and so on are often the respectable ritualistic facade for this flight from responsibility. It is a counsel of expedience which is sanctioned by the implicit consensus of those who, in one way or another, benefit from the present state of affairs.

The social scientist is stragically located and relatively autonomous. He is relatively free in his selection of topics for discussion and investigation. Social scientists are collectively relatively free to establish criteria both of significance and of evaluation for their products. The typical social scientist is relatively well protected from pressures exerted by the surrounding community. It is potentially in the nature of his activity to acquire information which has a relevance to the tasks of citizen and policy maker. Thus, he can play an extremely important role in a society which, at least in theory, depends on accurate information about and an understanding of social matters which are essential to the public interest. If he fails to use his freedom and his strategic advantages in a way which serves that interest, then this, while not perhaps calamitous, would harm the community.

5.

Do American social scientists fail to meet their responsibilities? How accurate is Mills' portrayal of the present state of the American social sciences?

First, it must be emphasized that though Mills claims to be describing the social sciences generally, including economics, political science, social psychology, anthropology and even history, the argument and evidence presented in the book obviously relate primarily to sociology. On the other hand, Crick's book may be taken as bearing out at least part of what Mills wants to claim about political science. For whatever the defects of Crick's book may be, the diagnostic portions contain much evidence for the contention that political scientists all too often proceed without examining their presuppositions, and in an abstractedly empirical way. On the other hand, most of Crick's named targets are dead, or were never political scientists. Into the former category fall Arthur Bentley, and Charles Merriam. Into both falls John Dewey. And into the latter falls T. V. Smith. All of these men come in for a good deal of eloquent condemnation. In fact, except for the brilliant chapter on Harold Lasswell, and some mention of political scientists like David Truman, Crick does not identify many of his *living* targets. He does, however, identify many of those he specifically excludes — e.g. Hans Morgenthau, Carl Friedrich, Hannah Arendt, V. O. Key, Leo Strauss, Russell Kirk, Clinton Rossiter, and the late Franz Neumann. One can think of many other prominent political scientists who do not seem to fit any of the objectionable patterns of Crick's analysis — Herbert Dean, David Spitz, Andrew Hacker, Samuel Beer, James Meisel, James Pollack, and Roland Pennock, to mention a few — and others who seem to fall into some middle ground — Robert Dahl, David Easton, and Robert MacIver, again to mention only a few of the more prominent. I do not say that Crick has not got hold of some "higher truths" — truths about dominant tendencies which it is difficult to pin down in terms of detailed evidence. Certainly the abstracted empiricist tendencies and the unthinking liberal tendencies do exist, but how widespread and how dominant they are is not made clear. More evidence should have been presented. Certainly Harold Lasswell cannot be regarded as *the* representative political scientist. Thus, Mills' basic claims, while they do receive some support, do not receive very much support from Crick's book.

Is the evidence presented in Mills' book at least sufficient to indict sociology? I think that no clear answer emerges.

Mills has picked easy targets and used questionable methods. Though I think his critique of Talcott Parsons is essentially sound, it is not clear that Parsons has as much influence in sociology as Mills

insinuates. Moreover he is not entirely fair to the man whose work he regards as prototypical of abstracted empiricism — Paul Lazarsfeld. The paper on which Mills bases his most damaging criticisms is an unpublished manuscript which Lazarsfeld wrote many years ago. To criticise the statement given in that paper without considering Lazarsfeld's other works on such topics as public opinion and the academic mind is not very fair — to say the least.

Nor does Mills give much evidence for his claim that the tendencies he describes are overwhelmingly dominant within sociology. One can think of a large number of persons located at major universities who simply do not fit the patterns Mills rightly finds so objectionable. One prominent sociologist has put his reaction to Mills' book in the following way.

I take a dim view of half of professional sociology — and that is the half to which Mills' polemics apply. But the other half — that half represented by the most active, scholarly sociologists located in departments within the best universities, share Mills' biases. But they do more — they act on them.

Again, it seems to me that Mills has hold of some "higher truths" which he does not defend as responsibly as he should have done. His work belies his own canons of responsible behavior.

Still, Mills has undertaken a vital task, and what he has to say does have an important relevance to American sociology today. In the performance of this vital task there is no one who is bolder, more imaginative, more provocative, and who has brought to its performance a more impressive array of talents and resources. This does not justify the defects nor excuse the excesses of his polemics — but it does help us to see Mills in a less carping and a less mean-spirited perspective than do many of his detractors.

Taken together, Mills and Crick have not shown as much as they claim, nor argued as well or fairly as they should — but they have demonstrated beyond a reasonable doubt that there are important tendencies for irresponsible thought and action within American social science.

¹ Routledge & Kegan Paul Ltd., 1959.

² Oxford University Press, 1959.

³ *Political Education*, reprinted in *Philosophy, Politics, and Society*; ed. by Laslett, Blackwell, 195.

⁴ It is interesting to compare a conception of social science advanced by a British sociologist, Peter Winch, with the conception which is implicit in Parsons' work.

In his book, *The Idea of a Social Science* (Routledge and Kegan Paul, 1958) Winch tries to show that L. Wittgenstein's philosophical views have profound implications for the social sciences. Winch argues that the rule-governed aspect of human activity, which is what Wittgenstein stressed in his later work, is what needs to be made central to social investigation if the scientist is ever to have a real understanding of human society. Winch's argument, stripped to essentials, is: Sociologists aim to describe social regularities. The idea of a social regularity presupposes criteria which enable us to determine whether two objects are the same sort of thing. But Wittgenstein has shown that what is to count as the same or similar things depends on human conventions or rules. E.g. Two instances of human activity will count as prayer only relative to certain criteria for identifying prayer. Moreover, in understanding prayer or any other social activity something more is required. Unlike the physical scientist the social scientist tries to understand persons who themselves follow rules. Thus, to understand any social phenomenon, e.g. prayer, one must have knowledge of how the actors themselves conceive their activities. But to understand their activities in this way, one must have a feeling for them. E.g., the "sociologist of religion must have some religious feeling if he is to make sense of the religious movement he is studying and understand the considerations which govern the lives of its participants." At the same time the social scientist must be disinterested — that is, uncritical of the institutions he is studying. It is at this point that the social scientist must become philosophical. For, "to take an uncommitted view of... competing conceptions is peculiarly the task of philosophy; it is not its business to award prizes to science, religion, or anything else. It is not its business to advocate any Weltanschauung. In Wittgenstein's words, 'Philosophy leaves everything as it was'." 103.

Thus, for Winch, the movement from emphasis on rule-governed aspect of human activity to the conservative implication, which Mills describes, is quite direct.

⁵ Morgenthau, Hans, *Scientific Man vs. Power Politics*, Chicago U. Press, p. 125.

⁶ *The Case for Modern Man*, Harpers Bros., pp. 102—108.

⁷ In *Aspects of Human Equality*, ed. by L. Bryson.

⁸ See also his books *The White Collar Worker*, *The Power Elite*, *The Causes of World War III*.

CAN SCIENCE JUSTIFY AN ETHICAL CODE?

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The attempt to utilize the methods of science to justify one ethical code as opposed to another has the advantage of avoiding the dogmatism and question-begging techniques characteristic of many traditional ethical theories. However, such attempts are invariably involved in value reductionism, leaving normative terms bereft of their normative import. Science is related to ethics in a number of important ways, but not in the sense that inductive evidence can justify one standard of right conduct as opposed to others.

I

Recently there have been a number of efforts made by philosophers to ground ethics on a scientific basis. What they mean by "grounding ethics on a scientific basis" is that the scientific method or the method of induction can be relied upon to settle ethical disagreements and to provide certain criteria by virtue of which one ethical theory is shown to be superior to, that is, more justified than, all others. Advocates of this scientific approach to ethics argue that we can have and in fact do have probable empirical knowledge of the truth or falsity of some normative ethical statements. Certain empirically discoverable characteristics are good criteria of the normative characteristics which constitute rightness. This, it is argued, can be shown to be true by the scientific method, namely, by presenting evidence which shows that the possession of certain empirically discoverable characteristics by an act is both necessary and sufficient to the possession of the normative characteristics which justify the choice of the act. The rightness of an act, then, is an empirical property which can be detected with probability by the method of induction. In other words, the use of the inductive method enables one to discover not only those characteristics which make right acts right, but also the acts which have those characteristics.

This being the case, it is then argued that the scientific method provides us with a common and agreed upon procedure for deciding between rival ethical theories. All that needs to be done is for one to carefully formulate a list of those acts which have the empirical properties which are necessary and sufficient for those acts to have the normative characteristics which constitute rightness. Then one need only examine those acts which rival ethical theories set forth as being right and see whether they have these necessary and sufficient characteristics. This can be done by the use of the scientific method. Professor Charles A. Baylis, one of the advocates of the scientific approach to ethics, puts his case in this manner: "As long as rival ethical theories are put forward as definitions or as dogmas, or as rules to be accepted 'only if you wish to play the particular game of morality that they govern', it is hard to find sound reasons for one view and against another. But once we regard these alternative views as rival hypotheses we can test them against the acts which our identifying methods have classified as right by determining whether or not the characteristics specified by the hypothesis as right-making are common and peculiar, that is, necessary and sufficient, to all acts so classified. If so the hypothesis is confirmed; if not it is disconfirmed."¹ This testing of an ethical "hypothesis" concerning right conduct is to be done in exactly the same manner as one would test a particular scientific hypothesis.

Baylis is no doubt correct that differences in ethics cannot be resolved as long as opposing ethical theorists set forth their normative criteria as arbitrary and unjustified definitions. The history of ethics shows that this has been the rule rather than the exception, and the scientific approach to ethics is a genuine attempt to avoid the sort of stalemates that have existed in ethical theory because of this dogmatism. In spite of this ideal which the scientific approach to ethics espouses, however, there are difficulties confronting it which, to my mind, tentatively call for a negative answer to at least one formulation of the question, "Can science justify an ethical code?" In this paper we will examine one of the most recent attempts to ground ethics on a scientific basis, that of Charles A. Baylis in his *Ethics, The Principles of Wise Choice*, New York, Henry Holt and Company, 1958. We will argue that Professor Baylis fails in his attempt to "base ethics on science" in the sense of utilizing the inductive method to justify one normative standard as opposed to others.

II

Professor Baylis is the more sophisticated of those who advocate the use of the scientific method as a means of discovering an ethical norm. He is opposed, and justifiably so, to any arbitrarily defined norms such as are found in much of traditional ethical theory. Such a procedure not only necessarily involves stalemates between rival ethical theories, stalemates which from the nature of the case cannot be adjudicated, but it also involves the fallacy of *petitio principii* or begging the question. Baylis argues that the scientific approach avoids both the stalemates and the question begging that has characterized most of traditional ethical theory.

What is required is showing by the use of the inductive method that the presence of certain empirical characteristics is both necessary and sufficient to the presence of normative ones. The presence of normative characteristics justifies the choice of the act which has those characteristics. This, however, does not mean that normative concepts are to be defined purely in terms of empirical ones. As Baylis puts it: "No observable characteristics are identical with normative characteristics."² Thus Baylis feels the force of Moore's "open question" argument and consequently tries to avoid any sort of value reductionism. "To drop these normative implications of the term "right" and define it entirely in terms of characteristics whose presence or absence is clearly discoverable by scientific procedures is to sell out our normative interests for a mess of pottage."³ What Baylis means is that any naturalistic definition of "right" (for example, that an act is desired, that it pleases, or that it is customary) will not tell us whether or not to choose it or perform it. Any definition of "right" must be in terms of some normative concept. Although normative terms are not "inherently indefinable . . . within any system that is not circular at least some normative term must be left undefined."⁴ Baylis chooses "preferable" as the undefined normative term in his system of ethics. "Preferable" is to mean "worthy of being preferred", not "capable of being preferred." The inductive method is to be used on any occasion when a moral judgment like "x is right" or "x ought to do y" is uttered. This judgment, rephraseable as "x is preferable to any other alternative in these circumstances," can be verified or falsified scientifically by discovering whether or not the act possesses those empirically discoverable properties which are both necessary and sufficient for that act to be preferable. It is not only the case, Baylis claims, that acts with these necessary and sufficient properties consti-

tuting rightness or preferability can be empirically discovered, but it is also the case that the use of the scientific method will enable us to discover those necessary and sufficient properties which constitute rightness or preferability.

Professor Baylis prescribes three steps for discovering both right acts and normative characteristics. First, certain ideal observational conditions must exist when choices of preferability are made. Secondly we must accumulate a list of acts which are chosen as preferable under these ideal conditions for making preferences. Thirdly, we must examine those acts chosen as preferable under ideal observational conditions and find the characteristic or characteristics necessary and sufficient for these acts to be right.

The ideal observational conditions for choosing include three factors: "(1) The preferrer is conscientiously seeking to predicate 'worthy to be preferred' or some synonym of it correctly. His dominant motive is to choose what is worthy of choice. He is moral. (2) He is aware of all of the significantly relevant features of the various possible alternatives. (3) In his consideration of these he makes no significant error of judgment."⁵ Baylis states these same conditions in this manner: "An honest preference of someone who is skilled in noticing relevant features of the various alternatives, and who is skilled in drawing inferences from the observed data, has a relatively high probability of being a preference for an alternative that is in fact worthy of choice."⁶

There is, however, one obvious difficulty with the statement of these conditions. The notion of "significantly relevant feature" is quite crucial, but Professor Baylis leaves it undefined. The most that we are told is that we learn from past experience what features are most likely to be relevant. This is no doubt true since we learn practically everything we know from past experience. But what is meant by "significantly relevant feature" must be specified. The phrase itself seems to be normative and not merely descriptive. It is clear, however, that if "significantly relevant feature" is defined in terms of consequences, then the question at issue, "What is meant by a normatively preferable act?", has been begged in favor of a teleological as opposed to a formalistic theory. This, I am sure, Professor Baylis wants to avoid and it behooves him to define "significantly relevant feature" more clearly.

Suppose, however, that we are through the first two steps that Baylis suggests. We have a list of acts which are preferred under ideal con-

ditions as specified above. We must now discover inductively what characteristic or characteristics, necessary and sufficient, those acts have which make them right. We must inductively test rival ethical theories, asking if the characteristic or characteristics specified by each are necessary and sufficient for the rightness or normative preferability of acts. How is this to be done? An example which Professor Baylis provides makes clear the testing criterion. He remarks: "The view that conforming to the agent's conscience is a guarantee that an act which so conforms is right, is shown with high probability to be wrong, by the fact that some moralists, allegedly including Tolstoy, said that their conscience would not permit them to resist by force a ruffian who had entered their home and was attacking their daughter. The *controlled preferences of almost all thoughtful people* indicate the preferability of resistance in such a case, and show such resistance to be probably right to a very high degree."⁷ Baylis's standard or test for discovering whether a particular act is right and the characteristic or characteristics necessary and sufficient for rightness is whether that act is a "controlled preference of almost all thoughtful people." "X is right" or "x is normatively preferable" means "x is preferred by almost all thoughtful people under ideal observational conditions (1), (2), and (3)" specified above. What can we do if some of our preferences seem to be wrong? "All we can do then is re-examine the preferences that led us to list the act as right in the first place, to examine other preferences in regard to acts of this kind and see if we need to revise our judgment as to the rightness of the act or kind of act in question." What do we look for in this re-examination and revision of our moral judgments? We simply discover if the act preferred conforms to the "controlled preferences of thoughtful people."

This re-examination and revision of our moral judgments can also be done by discovering whether the acts preferred have the characteristic of being probably productive of the greatest good of any possible alternative. Baylis argues that this characteristic is both a necessary and a sufficient condition for the rightness of any act. This standard is that of ideal or agathistic utilitarianism. This thesis of ideal utilitarianism is, he says, "most confirmed by the evidence available today. . ." Ideal utilitarianism as a hypothesis "has been confirmed to a higher degree than its rivals." What is meant by confirmation? Simply that under controlled or ideal conditions for making choices among alternative acts, almost all thoughtful people choose acts which have the property of being probably productive of as much good as any possible

alternative. This is what Professor Baylis means by an empirical verification of an ethical norm.

A right act or a normatively preferable act then is any act which is (1) probably productive of as much good as any possible alternative, or any act which is (2) preferred by almost all thoughtful people choosing under ideal conditions for making preferences. Since Baylis uses (2) to justify (1) then (2) and (1) are apparently not equivalent, although he seems to hold that any act that conforms to (2) conforms to (1). It would seem, however, that the issue of whether almost all thoughtful people under ideal conditions for making preferences always choose acts which conform to the standard of ideal utilitarianism would be an empirical matter. One would have to find thoughtful people, put them in ideal conditions for making preferences among alternative courses of action, and see what they do in fact prefer.

In any case since Baylis appeals to (2) to justify (1), (2) is his ultimate normative appeal; and we must ask how he justifies this norm since his contention is that no norm in ethics is to be arbitrarily accepted, but rather scientifically justified. Why should we do that which is "preferred by almost all thoughtful people under ideal conditions for making preferences"? Baylis, in his attempt to justify an ethical norm (that of ideal utilitarianism) by the scientific method must now justify scientifically the norm that it is right to perform acts which are preferred by almost all thoughtful people under ideal conditions for making preferences. Such a scientific justification is not forthcoming. Professor Baylis leaves us with a normative principle which has not been scientifically verified in any sense, namely, "we ought to perform acts which are preferred by almost all thoughtful people under ideal conditions for making preferences." He has not accomplished the task he set out to do. He has simply invoked one "ought" (2) to justify another "ought" (1).

I mentioned earlier that Baylis tries to avoid value reductionism. However, it is not clear that he does so. Ideal utilitarianism (1) is justified by reference to his supposition (2) that we ought to perform acts which are preferred by almost all thoughtful people under ideal conditions for making preferences. Now if a right act or obligatory act is defined in terms of any act preferred by thoughtful people under ideal conditions for making preferences and if the latter is viewed as simply descriptive (he seems to treat it as a descriptive phrase), then Baylis is involved in value reductionism. Of course, Moore's "open question" argument immediately applies. That is, if any descriptive

predicate is to express the meaning of "right", then it must be nonsensical to ask whether what possesses this descriptive property designated by the predicate is really right. If, as Baylis claims, "preferred by almost all thoughtful people under ideal conditions for making preferences" were the meaning of "right" in its moral sense, then to ask of any act which conforms to the "controlled preferences of the thoughtful" whether it is really right would be nonsensical. But clearly such a question is quite meaningful.

It is true that if "controlled preference of almost all thoughtful people" is a descriptive predicate, and if all ethical predicates are ultimately definable in terms of this descriptive predicate, then the answers to the two questions with which Baylis is primarily concerned, namely, (a) Can ethical disagreements be resolved by empirical or scientific methods? and (b) Can science provide us with an ethical norm?, must be answered affirmatively. For surely the method of induction can provide us with probable empirical knowledge of what constitutes the "controlled preferences of almost all thoughtful people." This may be difficult, but in principle at least it can be done. And having this empirical knowledge, we can then resolve ethical disagreements simply by discovering if the preferences or acts of ethical rivals agree or disagree with "the controlled preferences of the thoughtful."

The problem with this account, however, is the same that confronts any form of value reductionism and Professor Baylis himself recognizes this problem in connection with other naturalistic accounts of "right." The problem is that a complete naturalistic definition of "right" leaves the term bereft of its normative import. Using one of Professor Baylis's own examples, if "x is right" means only that "x is customary," then knowing that x is right will not tell what we need to know about x, namely, whether or not we ought to choose x or perform x. "Right" in this case has lost its normative implications. The same problem confronts Professor Baylis's naturalistic account of "right." If "x is right" means only that "x is a controlled preference of almost all thoughtful people," then knowing that x is right will not tell us what we need to know about x, namely, whether or not we ought to perform or choose it.

The above criticisms seem to me to be applicable to Baylis's position. However, there is a way in which he can avoid value reductionism, and it is clear that he does want to avoid a naturalistic account of "right". This can be done via the concept of "thoughtful" or "rational" in norm (2) above. Suppose that the statement "a right act is

any act which is preferred by almost all thoughtful people under controlled observational conditions for making preferences" could be shortened to "a right act is a rational act." If then the concept "rational" is left undefined, value reductionism is avoided, for surely "rational" is a normative concept. However, it is avoided at the expense of precluding a scientific justification of an ethical norm — in this case that we ought to be rational. Baylis's appeal to the "thoughtful" or the "rational" in fact seems to me to be not very far from Aristotle's appeal to "practical wisdom" or the "man of insight," Mill's appeal to the "wise man," or even Ross's appeal to the "mature and experienced man." His appeal to "controlled preferences" is also close to the dispositional analysis of moral judgments, advanced on occasion by Hume and more recently by Firth and Brandt,⁸ in which a moral judgment, for example, "Stealing is wrong," would be defined as "Stealing is such that it would be disapproved of by an impartial, intelligent being who was informed on the general results of stealing." The property of "rightness" is here defined in terms of an attitude of approval, "wrongness" in terms of an attitude of disapproval. Baylis's appeal to the "controlled preferences of the thoughtful" is very close to this appeal to an "impartial, intelligent spectator."

III

Our conclusion concerning Professor Baylis's attempt to "base ethics on science" is that he has failed to show how the use of the inductive method can justify one normative principle as opposed to another. It is true, however, that ethics can be based on science in a number of senses. For example, (1) Science can provide a descriptive account of what sorts of things or experiences are valued and (2) who places this value (groups or individuals) on these things or experiences. (3) Science can provide probable empirical knowledge of what causes persons or groups to value something. (4) Science can provide a descriptive account of the genesis and causes of societal organization. (5) Science can provide probable empirical knowledge of which means will best lead to certain ends and the general consequences of conformity to certain rules of conduct. (6) Science can provide probable empirical knowledge of the manner in which choices of means are determined by choices of ends and the manner in which choices of ends are determined by choices of means.

No doubt there are also many other senses in which ethics may be based on science. Anthropological data about man's primary needs

are relevant to ethics. The data provided by the psychoanalyst are also relevant to ethics. Information from economic theory and practice is also quite relevant. However, all of these descriptive data have what may be called "means-ends value". That is, they provide probable empirical knowledge of what sort of consequences generally follow from certain practices. They provide us with knowledge of the instrumental value or disvalue of various courses of action. Given a certain valued or valuable state of affairs, these descriptive data provide us with probable empirical knowledge of the best means to this end. However, these data do not show us what ends or goals are *desirable*, *valuable*, or to use Professor Baylis's phrase, *normatively preferable*. They do not provide us with a standard of right conduct.

In one sense of the question, "Can science settle ethical disagreements?", the affirmative answer of Baylis's theory is correct. If there is disagreement between rival ethical views on empirical data or upon the empirical premise of an ethical argument, then, in principle at least, the scientific method can be used to settle such disagreements. If, for example, two disputants both accept the norm of hedonistic utilitarianism, but disagree on whether a particular act will probably produce the greatest pleasure for all those concerned, this disagreement, in principle at least, can be empirically resolved by a careful examination of the present circumstances surrounding the act and the general effects that acts of this type have had in the past in similar circumstances.

However, has Professor Baylis's scientific approach to ethics shown how science can rationally settle a dispute between rival ethical views when this dispute is totally normative? I think not. In this most important sense of the question, "Can science settle ethical disagreements?", a negative answer is appropriate. Clearly it is both a logical and empirical possibility that two rival ethical theories will disagree, not merely on what means are best to attain an end or goal, but on the *worthiness* of ends or goals. Professor Baylis's scientific approach to ethics has not shown how the use of the inductive method can justify one normative principle (statement of the worthiness of ends or goals) as opposed to others. He ends by simply defining right conduct in terms of a naturalistic account, namely, "the controlled preferences of the thoughtful."

On Professor Baylis's theory it is true that many ethical expressions are justifiable. For example, the judgment that a particular act *x* ought not to be done may be justified by reference to the descriptive

minor premise that x is an instance of stealing and the normative major premise that all acts of stealing are wrong. However, if we ask why stealing is wrong, we can be given only a similar argument with another normative principle as its major premise. In the last analysis, Professor Baylis is forced back to a moral principle which is simply asserted and not justified, "the controlled preferences of the thoughtful." Perhaps the answer to the question we have been considering lies simply in the recognition that it is analytically true that *ultimate* moral principles cannot be justified.

¹ Charles Baylis, *Ethics, The Principles of Wise Choice*, New York, 1958, p. 248.

² *Ibid.*, p. 246.

³ *Ibid.*, p. 235.

⁴ *Ibid.*, p. 238.

⁵ *Ibid.*, pp. 242—3.

⁶ *Ibid.*, p. 245.

⁷ *Ibid.*, p. 249. My italics.

⁸ Roderick Firth, "Ethical Absolutism and the Ideal Observer," *Philosophy and Phenomenological Research*, vol. 12, (1951—2) and R. B. Brandt, "The Status of Empirical Assertion Theories in Ethics," *Mind*, vol. 61, (1952).

DISCUSSION

SHOULD POETRY BE CONSIDERED A KIND OF DISCOURSE?

Isabel C. Hungerland, *Poetic Discourse*, University
of California Publications in Philosophy, Vol. 33,
Berkeley and Los Angeles 1958.

Much of the most typical "New Criticism" has been strongly rationalistic; especially critics who follow the line of I. A. Richards emphatically hold that one can reason about everything in poetry. The techniques developed within modern analytical philosophy have properties which make them well adapted to reconstructive criticism of such reasoning about poetry, for which purpose Professor Hungerland uses them with evident success. I shall give an account of her brilliant book, and after some critical remarks proceed to a discussion of the fruitfulness of her approach.

I

The book falls into two main parts of three chapters each. The first, the general part, aims at "throwing some light on the artistic use of language" (p. 106)¹ by differentiating concepts which may serve to distinguish poetry from "other kinds of discourse" (p. 5). Considering 'poetry' as an undergroup of 'language' she seeks to illuminate it by application of linguistic concepts, which she examines for their usefulness in characterizing the special function in poetry of the features of ordinary language. Everyday language and common-sense reflections constitute the basis upon which she attempts to demonstrate that poetry is a subclass of language which "specializes" the language, but "in ways that are not at all foreign to ordinary life" (p. 46). The difference between poetry and the remainder of language is sought in differences between "language situations" (p. 44), and she tries to sketch a map of the field by the indirect way of analyzing critically some prominent views of contemporary literary theory in the USA.

Professor Hungerland sets out with a condemnation of the classification of language as 'descriptive' and 'emotive' meaning, and states: "Attempts to deal with the problem of scientific discourse versus literature or fiction have been, I think, singularly unilluminating" (p. 4). Not so much proposing alternative classification, she denounces the very idea of classifying in mutually exclusive classes (pp. 5, 10): "Attempting to divide logically the field of meaning is rather like attempting to divide logically the field of manners." (p. 6) Her cautious use of the 'family'-concept (pp. 4ff, 9ff, 31) and 'word-game' (pp. 16ff, 61) reminds one of a certain influence from the later work of Wittgenstein. Otherwise her frame of reference is rather limited; the philosophical sphere in which she moves is indicated by the way she uses 'philosophy' without qualifications in cases where the statements seem most relevant for certain established trends in American semiotics (pp. 14, 48, 51, 107, 111).

The emotive-descriptive dichotomy is found useless for investigation into the relevance of syntactical form to tone (pp. 8—13). The suggestive power of poetry is explained in terms of connotation and association (pp. 14ff), whereupon she discusses several commonplaces of literary criticism like emotion in poetry and how it differs from ordinary emotions (pp. 18ff.), how far personal experiences of readers determine poetry-reading (pp. 24ff.), and how modern critics overestimate ambiguities (pp. 26ff). In her own view poetry cannot be singled out in terms of the predominance of some special kind of meaning (pp. 12, 43): "there is no such thing as a poetic language." (p. 13) Instead of trying to define the common concept of poetry she substitutes a broader one, this being the variety of situations in which we are occupied with all those features and workings of language which she deals with in the book (p. 46). She seeks to enlighten poetry by discussion of different kinds of uses of language, but "None of these differences justifies us in attributing to the professional literary man some single and simple aesthetic faculty." (p. 47) The kind of "knowledge" found in poetry is compared to the preverbal and prepropositional insight of a skilled horse-trainer (p. 54), and the difficulties of appraising poetry found to be practical, not philosophical in nature, being in certain respects "very much like the unproblematical appraisals we make of skills like horsemanship, and of objects like automobiles." (pp. 64ff) Through the multiplicity of criteria of worth she establishes the current appraisal-liking distinction, and holds that the "perception and description of works of art

is a somewhat special development of ordinary "objective" perception and description of things." (p. 88); only the predominance of certain "core-criteria" marks out the evaluation of poetry.

The remainder of the general section is devoted largely to criticism of some additional literary doctrines. *The second part* of the book deals with figurative language, symbols, and problems of evaluative interpretation. Figures and symbols are approached by a consideration of their use in everyday discourse (p. 108), "a certain kind of experience which may be formulated in language in a variety of ways." (p. 136) There is the "seeing one thing as another", the "treating as" and the "suggestion", three features of human behaviour which explain the use of figures (pp. 113ff), and the properties of figures which give them potentiality for effective use in poetry are: "concreteness, condensation, suggestiveness, and the thwarting of the customary response to language" (p. 119). By way of examples she shows that no classification can make up for detailed examination of the figure's function in context (p. 129); and states the illegitimacy of taking imagery as a criterion of excellence in poetry (p. 134).

Professor Hungerland takes upon herself the useful task of approaching the topic of symbols without awe, and defines her use of 'symbol' with the help of the concepts 'transference' and 'suggestion.' As figures were said to be "a kind of linguistic treatment of one thing as another" (p. 115) symbols are said to be established by transference of thoughts and attitudes from one object to another (p. 138), whereby "almost anything can become a symbol of something" (pp. 138—9). She proposes to use the term 'symbol' regardless of the awareness of the receiver of the symbolic relation (p. 139). Parallel to her rejection of the unreserved appraisal of ambiguity and imagery she here exposes the naivety of the "symbol-hunting reader of today", and shows why psychoanalytic symbol-interpretation "is not so much misleading as incomplete." (p. 149) Archetypes are devaluated (pp. 151ff), and several kinds of "mystification" disowned.

The chapter on interpretation states the legitimacy of various approaches to poetry, discusses the "intentional fallacy" (pp. 161ff.) and the necessity of knowing the poet's intention in order to evaluate his skill. The poet's own interpretation of his work is said to be "privileged, but not authoritative" (p. 176). In conclusion she states some distinctions between (1) incorrect, (2) implausible interpretations, and (3) a set of "alternative ways of explaining and seeing the work, none of which are wrong or implausible or odd." (p. 175).

II

If this account of Professor Hungerland's book gives the impression of being exceedingly complex, I should like to plead in justification that the book is complex too. The complications derive both from the complexities of the matter, *and* from the method adopted in dealing with them. Besides, some explanation can perhaps be found in the acknowledgement to an unusually large number of professors for "helpful suggestion"?

The method consists for one thing in selecting single lines of reflections, and not pursuing them exhaustively, but distinguishing problems and provoking questions (p. 177). To bring out the crucial aspects, selected misunderstandings of modern criticism are tackled. Thus the very disposition of her study is oriented according to the emotive-descriptive dichotomy which she repudiates in its different manifestations. And it is surely a most original and stimulating treatment of the problems involved, as I have tried to show in the citations made above. No systematic theory is sought, and the attitude is altogether a critical and negative one. Her intelligence shoots off sparks against current misconstructions in literary theory, and the reader is dazzled as they kindle and consume the adversary. Watching this spectacular display of spiritual energy one wishes, however, that some of it had been devoted to more constructive purposes: to a coherent and comprehensive sketch of the *better* way to approach poetry. Such an obligation might induce the author to let the sparks form constellations of their own, whereby her view would certainly be more easily accessible. Such a "positive" attitude might also better co-ordinate matters that are now somewhat haphazardly strewn around as the battle fluctuates to and fro. Then the other part in a dialogue like this will tend to influence unduly the disposition and affect the standard of the response. Often the mediocrity and banality of her adversaries affects her exposition badly, a trap one easily falls into when operating with common-sense notions. Professor Hungerland feels challenged at every step to take up arms against clusters of other views, a procedure which weighs down her own presentation, and makes it more difficult for her to reach her own point. As soon as she is on her way to a more comprehensive view she drops the line of research at its most promising point to follow some real or imaginary adversary (e.g. pp. 7, 47, 48, 86). The reader gains from this procedure several satisfying executions and commentaries upon representative faults of criticism. But not all her victims are equally interesting. At times

she seems to tailor easy generalizations on the opponents (e.g. pp. 57, 86, 101, 152), constructing schools well fitted as objects of criticism, under names like "Naturalism" (pp. 90ff, 99, 168ff.) or "Idealism" (pp. 105, 169), for the most part without reference to anybody in particular or to any identifiable theory. At one of the places where she does specify by explicit reference we find a remark to which I willingly subscribe: "The demolishing of straw men is instructive only so far as live models for the effigies can be found." (p. 162) It seems as if the occasional presence of this sound idea in her mind reminds her of the use of reference, a phenomenon which has several counterparts in her book. Cf., for example, the attitude to the paraphrase-problem on pp. 43, 127, 176, the fault of thinking the problems *through* gives inconsistency, and near to contradictory statements, in my opinion because her theoretical background does not allow her to give a satisfactory account of the problems. But the limited task she has taken upon herself is to "sketch a map" and find an approach and she repeatedly makes modest reservations: "More questions have been raised than answered in this chapter." (p. 60) Actually, her extensive use of extraordinarily cautious reservations (pp. 11, 77, 99, 106, 177) has, at least upon me, some of the effect of a double negation, and as she at least wants to stress an approach to poetry that does not lead up a blind alley (p. 60), and to find concepts that are instrumental in the analysis of poetry, I shall briefly discuss these modest, but not indifferent pretensions.

III

The book is clearly a very competent attempt to provide a theoretical foundation in a field where it is highly needed, and it deserves discussion as to whether it is recommendable as a basis for further work. To this end I propose that we question her basic orientation, and even what to her seems self-evident. The book opens with this sentence: "That poetry is discourse and that it differs in some ways from the discourse of science is a proposition which few, if any, would dispute." Actually it is possible to dispute this proposition and the attitude it reveals. In doing so I place myself outside of the philosophical sphere in which she operates, within which hardly anybody would deny that poetry is discourse. And yet it is not narrow, this philosophical tradition. One of its great names is I. A. Richards, whose theory Professor Hungerland attacks for some faults, but she still conducts her campaign from the same general platform that Richards

established in the twenties. The underlying assumption is that all problems of literary analysis can be solved by recourse to linguistic analysis, because, as we have seen, poetry is merely a subdivision of language. This latter proposition is even considered as rather selfevident.

Now there is no point in under-rating the valuable contribution of linguistics and semantics to literary theory; much modern criticism testifies to the successful application to literary analysis of certain techniques developed under the study of language in general. If one does not realize the limits of this instrumental value, the failure can be described as the fallacy of attributing too much importance to such elements of poetry as lend themselves best to techniques developed in another field of research.

Look at the programmatic opening sentence quoted above; "discourse of science" holds an important place in it, indicating that the general concept of language is built with more regard to its function in science than to poetry. This attitude, which Professor Hungerland shares with the many who are "inside" her sphere, I shall here question as one of those who are outside, and my view will certainly both profit and suffer from that special point of view.

Following a kind of Gestalt-theory of poetic experience we say then that the parts of a poem derive their meaning from an organic whole. A description that pretends to say something adequate about a work of art as it is present in our aesthetic experience must have a conceptual framework which reflects the structural relations within such a whole, and do justice to the composite organic structure by describing the relations that are constitutive for the aesthetic experience. Now a central problem for inquiry in this field as in so many others is what elements to choose for description. Professor Hungerland enumerates elements other than those found in ordinary discourse, and establishes their importance (e.g. pp. 127, 176), but she treats with her conceptual apparatus only those which are most easily recognized in ordinary language. This is natural, as she sees no point in isolating poetry as a world of its own. But any theory which builds its concepts upon a reality other than the aesthetic, a cognition of objects other than the objects of one's proper field, runs the risk of giving the literary analysis a bias, and here it is of little help that the inquirer, as is the case with Professor Hungerland, evidently has some intuition of what poetry is, when the conceptional framework at her disposal does not allow her to do it justice. The concepts must have at least the potentiality to reflect the structure of the experience which constitutes the

object of the inquiry. What is needed is a theory of poetry as an object of knowledge *sui generis*, a possibility Professor Hungerland ardently rejects: "If . . . one rests on the statement that the field of art is *sui generis*, one ends philosophizing before one has made even a good start." (p. 64).

I shall try to point out some imperfections of the book if judged on the basis the theory I myself accept, that poetry is an autonomous mode of symbolic representation. I can mention her arguments against the 'unity'-concept, where she wants to substitute a "machine" for "organism" in the expression "organic unity", and finds unity inconsistent with the diffuse organization of a long narrative poem (p. 76); and her arguments against the Brooks-Warren principle of harmony between the verse and the rest of the poem (p. 80). Her refusal to consider the power of poetry of direct symbolization is evident from her account of how literature may reveal insight even in cases where the verisimilitude is reduced *ad absurdum*, as in caricature: because a deviation from our customary perception can open the way for new noticings (p. 60). The kind of 'knowledge' found in literature is considered as more than emotion, but less than science (pp. 44ff., 61): "What literature can do for the theorist is to direct his attention . . ." (p. 59) She denies the existence of "aesthetic emotion" (p. 23), and writers are said to depict emotions "plainly, by depicting characters living in a certain way in certain situations". (p. 20) She brings arguments against those who tend to disparage paraphrase by referring to Yeats's writing of prose statements as a first step in the composition of poetry, disregarding the possibility that he might have the rest of the poem in his head (p. 44). She finds it difficult to "explain" the use of erotic imagery in religious poetry without a psychoanalytical or other theory explaining the connection between the two kinds of experience (p. 149, cf. p. 141).

This example leads us towards her proposition that there "is in the usual sense of the words not only one object, but one language". (p. 85, in connection with descriptions of paintings and of criminals) From such a monistic viewpoint it is not surprising that she finds no principal difference between the work of a poet and the writings of any one else (p. 47), and we understand that her efforts to account properly for poetry as an antonomous mode of symbolic representation become enormous. I shall refer to one of the utterances which signal an awareness beyond what her theories allow: "in what language suggests and conveys, beyond what it conventionally means, we are again in a borderline area, where right and wrong, correct and in-

correct handling or construing of expressions, shades off into an area where these epithets do not apply." (p. 175. cf. p. 6, where the meaning of linguistic expressions is defined in terms of rules) In this "border-line area" there reside, however, central poetic meanings, which she is precluded from accounting for because she not only loftily dismisses all kinds of "mysticism" (pp. 5, 43, 113.) but firmly refuses to consider poetry as a reality *per se*. Her philosophy permits no separate ontological status for the literary work of art.

From another viewpoint the full experience of poetry is seen to contain specific poetic features, i.e. those which transcend those accounted for in analysis of ordinary language. Of course such features are accessible by direct intuition, and, as we have seen, they are also experienced by Professor Hungerland. They may be described in many ways on a pre-scientific level, but Professor Hungerland puts forward the indeed laudable claim to include them in a logically coherent exposition formed upon a consistent theory of the mode of existence of the literary work of art. Then the exposition will reveal the qualities and the imperfections of the theoretical foundation. In this case the indicators are the occurrence of a certain inconsistency which is unnatural in an otherwise soberly intelligent discussion, and some instrumental insufficiency, as I have tried to show. Professor Hungerland has "les défauts des qualités" of her "school", and I suggest that she would do well to pass outside it and reconsider the problem of the mode of existence of the literary work of art. We need not take it as an ontological question in the traditional meaning, but as a search for the better conceptual tool among several possible of equally limited hypothetical validity. If she *insists* upon considering poetry as a kind of discourse that only "specializes, one might say, by making common forms of language function more effectively than they do in ordinary discourse, but in ways that are not at all foreign to ordinary life" (p. 46), then she runs the risk of forfeiting comprehension of important parts of poetry.

Poetic Discourse is a remarkably clever and stimulating study, and standard topics of aesthetics are treated with refreshing sobriety. Within the scope of its orientation it is a very able performance, but as far as I can see her way of seeing things derives from a school of literary theory which is no longer fruitful. And looked at this way the present book seems to be brilliance in a blind alley.

Knut Hanneborg.

¹ Page references are added liberally because the book has no index.

STUDENT PHILOSOPHICAL OPINIONS: A SURVEY

by

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Opinion surveys were taken in an effort to determine the philosophical beliefs of students beginning philosophy. Correlated sets were made of those who took the survey before and after a first course in philosophy; and opinion shifts noted.

The acquired information may be of interest to people in various disciplines. The authors tested the semantic usage of certain epistemological terms, the change in religious beliefs, the degree of consistency between general skepticism and particular knowledge claims. Finally, the authors proposed a consistency test for the measure of the extent to which a sample of students "clarified their beliefs," and attempted to measure their own pedagogical efficiency with reference to this test.

Introductory courses in many fields are notoriously difficult to teach. In philosophy, the difficulties are compounded. Beginning chemistry is taught without question as a preparation for more advanced work and beginning philosophy can of course be taught in the same way, but most students taking one or two classes in philosophy do not plan to major in the field. What does one teach in a philosophy course which is at once introductory and terminal? Many teachers feel that such a course ought to be more than a history of philosophy; that it ought to be a "core" subject in a liberal arts curriculum; that, in even more utopian terms, it ought to stimulate the student to begin "the examined life".

Several years ago, the University of Wisconsin department of philosophy appointed a committee to discuss ways of improving the teaching of introductory philosophy. The committee members thought it pedagogically important to ascertain the attitudes and opinions of beginning students toward some of the philosophical questions likely to be discussed during the semester. Presumably it would be these initial beliefs that would come under scrutiny if students did in fact take philosophy as a stimulus for self-examination. They thereupon drew up a list of what they took to be typical philosophical beliefs of freshmen.

Naturally one would not expect freshmen to hold initial positions on such philosophical issues as the nominalism-realism controversy. Nevertheless, it is not stretching a point to claim that beginning students have opinions which can be labelled "philosophical". They have, for example, ideas about the free will question. They have ideas about what can or cannot be known with certainty. They have beliefs about the nature of morality and religion. Their opinions are not well-formulated and are often incompatible, but this only emphasizes the need to determine their initial ideas. If students really think about their own beliefs, if what philosophers say really touches them, then one should expect certain clarifications of students' philosophical opinions.

The authors of this paper, both members of the University of Wisconsin committee, decided to design a series of opinion surveys with two aims: (1) to find out what some of the philosophical beliefs of students beginning philosophy actually are and (2) to examine opinion shifts that occur during the semester. We further hoped that a survey could be designed to gauge roughly the extent to which students "clarified" their beliefs. The clarification of beliefs was taken to involve at least the elimination of incompatible beliefs. Thus, if a student became more consistent after taking introductory philosophy, this could be interpreted as a sign that he had subjected his opinions to some examination.

A good teacher, it is true, can estimate with fair accuracy from class-room discussions and examination papers what his students are thinking. Sometimes, however, students do not say what they really think. Furthermore, a teacher has a tendency to overestimate the prevalence of what he takes to be an "outrageous" opinion. Consider an example: During the McCarthy era, writers for many newspapers and magazines joined the words "atheism" and "communism" with a hyphen. These writers undoubtedly had as their goal the conceptual merger of the two ideas they hated most. In teaching an elementary logic class, one of the authors found students who had difficulty understanding that "All communists are atheists" does not imply "All atheists are communists". These students had no difficulty seeing that "All whales are mammals" does not imply "All mammals are whales". He concluded that the newspapers and magazines had achieved their goal in good measure, and thereupon proceeded to bore his introductory philosophy students with lengthy disquisitions on the difference between atheism and communism. However, the opinion survey of students who were about to take an introductory philosophy course produced the following interesting results.

Question:

"Suppose one knew that all communists were atheists. Would it follow that all atheists were communist?"

- a. Yes, it would always follow that all atheists were communist.
- b. Sometimes it would follow and sometimes it would not.
- c. No, it would never follow that all atheists were communist."

Not one student checked *a*; 30 % checked *b*; 70 % checked *c*. Theoretically the "b" category was to eliminate those students who were confused about the logical sense of "follow" intended in the question. At any rate, the answers to the question indicated that valuable lecture time had been wasted in belaboring the distinction between atheism and communism. The lecturer had overestimated the prevalence of a view he considered outrageous.

* * * * *

The surveys were given over a two year period, the first in the spring of 1957 and the second in the spring of 1958. Unless we state otherwise, references are to the second survey.

Three separate sections of introductory philosophy are taught each term at the University of Wisconsin. Each section averages 110 students. The instructors are at liberty to choose the texts and otherwise conduct their courses. When relevant, we will mention the materials covered in the different sections. Two of the three sections surveyed in the spring of 1958 were taught by the authors. Insofar as possible, all instructors whose sections were being surveyed avoided during the course of the semester explicit references to any of the survey items. The questionnaires were presented at the beginning of the first lecture period and the end of the last. Students were informed that their answers would be anonymous and that they need not answer all or any of the questions asked. They were assured that their identities would not be known to the instructor, but were told that another survey would be given at the end of the semester and that we wished to match, individual by individual, the responses on both questionnaires. For this purpose, the first survey form was numbered, and students were asked to record the number so that they could write it on the second survey form. As expected, many students lost or forgot their numbers by the end of the term. These students took the second survey, but their forms were separated from the rest. Further, we asked students to state their age, sex, religion, fraternal affiliations, political prefer-

ences, *etc.*, so that it was possible to double-check the matching of the two surveys. Doubtful cases were eliminated. Although this reduced the sample size from about 330 to 211, we attempted to assure ourselves that opinion shifts found in the second survey would not be misleading because we had in part two different populations. Also, it was necessary in making chi-square significance tests and in estimating whether students became more or less consistent that this be done individual by individual, not *en bloc*.

Many of the original hypotheses advanced by the committee about initial philosophical beliefs were confirmed by the test results. One of these hypotheses, for example, was that students prefer teleological explanations to causal ones. The following item appeared on the questionnaire:

"Suppose one were asked why the ducks flew south this year. Which of the following statements would you consider the best explanation?

- a. Ducks always fly south in the winter.
- b. Ducks desire a warmer climate.
- c. Ducks have an instinct to fly south.
- d. No explanation is possible.
- e. Cold produced a change in the pineal gland which is located near the duck's brain."

At the beginning of the term, the responses by percentage were (a) 2 %, (b) 41 %, (c) 48 %, (d) 3 %, (e) 6 %. The majority favored either a teleological explanation or an explanation by instinct. It is of interest that the survey at the semester's end showed no essential change in belief. In all three introductory philosophy sections, students read part of Hume's analysis of causation in the *Enquiry*. The previous year, students in one section read the entire *Enquiry*, yet the earlier survey produced the same results. Many of the students who checked "c" or "d" at the end of the term could and did write excellent examination papers on Hume's analysis of causation. Nonetheless they did not accept the explanation of the ducks' behavior most in keeping with Hume's doctrine. An amusing result was the slight increase in one section of the number of students who checked "e". Perhaps they were influenced by Descartes' account of mind-body interaction.

Another committee hypothesis was that most students believe that what is true for one person may not be true for another. The first year

the surveys were run, 80 % of the students apparently believed at beginning of term that all or most truths were relative. At the semester's end, about 65 % were still of the same opinion. The authors noticed also that students strongly objected in class-room discussion to the view that the expressions "true for Mr. X" or "false for Mr. X" have no meaning. One of the authors advanced the hypothesis that students might be using the expression "true for Mr. X" as synonymous with "Mr. X believes . . ." and "false for Mr. X" as synonymous with "Mr. X does not believe . . ." This hypothesis was tested in the second year survey. The same item from the first year survey was included in that of the second year; it read:

"All truths are relative. What is true for one person could be false for another. What do you think?

- a. I think all truths are relative.
- b. I think most truths are relative.
- c. I think most truths are not relative.
- d. I think no truths are relative."

Initially, answers were as follows: (a) 20 %, (b) 63 %, (c) 12 %, (d) 1 %. (If the set of percentages given does not add up to 100 %, it is because some of the students did not answer the question.) The survey at the end of the semester yielded as results: (a) 16 %, (b) 40 %, (c) 15 %, (d) 26 %. Thus, even though there was a shift in the direction of *d*, 56 % of the students still held that all or most truths are relative.

To test further the results gained, the following items also appeared in the survey (they did not occur together, but were scattered throughout the questionnaire):

- (1) If someone believes that there are men on Mars, then the statement "There are men on Mars" is true for him.
- (2) If someone believes that there are men on Mars, then it is a fact that there are men on Mars.
- (3) Whatever *everyone* believes is true.
- (4) If someone does not believe that there are men on Mars, then the statement "There are men on Mars" is false for him.
- (5) Whatever *everyone* believes is false, is false.
- (6) If someone believes that there are men on Mars, then the statement "There are men on Mars" is true.
- (7) Whatever anyone believes, is true for him.
- (8) Whatever anyone believes is false, is false for him.

For each of the items, students could strongly agree, tend to agree, tend to disagree, or strongly disagree. The results at the beginning of the term were:

- (1) 76 % strongly agreed or tended to agree.
- (2) 99 % strongly disagreed or tended to disagree.
- (3) 91 % strongly disagreed or tended to disagree.
- (4) 89 % strongly agreed or tended to agree.
- (5) 85 % strongly disagreed or tended to disagree.
- (6) 93 % strongly disagreed or tended to disagree.
- (7) 77 % strongly agreed or tended to agree.
- (8) 84 % strongly agreed or tended to agree.

These figures tend to confirm the hypothesis that when a student says that the same proposition can be true for one person and false for another, he usually means something quite innocent: that the same proposition can be believed and disbelieved by different people at the same time.

In testing for synonymous usage, we could have presented a student with, say, the sentence "It is true for Mr. Jones that there are men on Mars", and then asked directly whether the sentence would express the same assertion if "It is true for Mr. Jones" were replaced by "Mr. Jones believes that ...". Direct questions concerning synonymity, however, may not reveal true linguistic habits; a subject is apt to become cautious and make distinctions between meanings which he does not make ordinarily when using a language. In our survey, we were primarily concerned with habitual beliefs, attitudes, opinions and usages. We therefore preferred indirect questions.

A committee hypothesis clearly falsified was the following: Most students believe that a person who doesn't believe in God or has no religious faith has no reason to be moral. The questionnaire item read: "If someone does not believe in God, then he has no reason to be moral." Students could (a) strongly agree, (b) tend to agree, (c) tend to disagree, (d) strongly disagree. The results for the initial survey were (a) less than 1 %, (b) 7 %, (c) 41 %, (d) 51 %.

* * * * *

So far we have been concerned primarily with initial philosophical beliefs held by beginning students. We shall now report on some shifts in student opinions that were discovered in the comparison between the initial and final surveys. Such information is important in many ways. It

becomes possible, for instance, to evaluate the effectiveness of a text or teaching technique in getting across a certain philosophical idea. To consider a case in point, two section instructors discussed the so-called "argument from design" during the first year the surveys were run. Instructor A spent more time on this matter than instructor B. His section read the whole of Hume's *Dialogues on Natural Religion*; B's section read a selection from Baron d'Holbach's book *Common Sense*.

The survey item pertaining to the design argument read: "The marvellous design and order in the universe are proof that there must be a God who designed the universe. Do you agree or disagree?"

Again the student had four options. He could (a) strongly agree, (b) tend to agree, (c) tend to disagree, (d) strongly disagree. The following table gives the results by percentage of students.

TABLE I

	Instructor A		Instructor B	
	<i>Before</i>	<i>After</i>	<i>Before</i>	<i>After</i>
a.	36 %	22 %	47 %	10 %
b.	35	42	23	24
c.	17	19	15	22
d.	12	17	15	44

If one combines category (a) with (b) and category (c) with (d), the picture becomes clearer¹. In A's section, there was an increase of only 7 % of students who rejected the design argument as a *proof* for God's existence. In B's section, there was an increase of 36 %. The following year, A and B both used the d'Holbach essay. In addition, A decreased the time spent in discussing the design argument. For the second year survey, the same item was used. This time, a third instructor's section was polled. This instructor (Instructor C) did not discuss any proofs for God's existence. Here are the figures for each section.

TABLE II

	Instructor A		Instructor B		Instructor C	
	<i>Before</i>	<i>After</i>	<i>Before</i>	<i>After</i>	<i>Before</i>	<i>After</i>
a.	27 %	12 %	29 %	7 %	20 %	18 %
b.	41	38	34	36	36	34
c.	24	19	27	28.5	27	32
d.	8	29	10	28.5	17	16

The second year, there was an increase of 16 % in A's section of students who rejected the design argument as a proof for God's existence; in B's section, the increase was 20 %; in C's section, the increase was insignificant — 4 %. Undoubtedly many accounts of these figures can be offered. An obvious suggestion is that A was a more able teacher during the second year. At any rate, after considering the survey results, Instructor A decided to use d'Holbach's essay in the future instead of Hume's.

This decision, however, raises a tricky pedagogical question. We said earlier that surveys can be useful in evaluating the effectiveness of a text or teaching technique in communicating a philosophical idea. In objection to the decision to change texts, one might argue that Hume was undoubtedly a greater philosopher than d'Holbach. Further, few philosophers would deny that Hume's *Dialogues* are more subtle and philosophically profound than d'Holbach's essay on the same subject. On this basis alone, it might be maintained, one ought to read Hume rather than d'Holbach. To push the argument further, one could add that an instructor who chooses texts and techniques solely on the grounds of how well they serve in changing student belief is little more than a propagandist.

In answer, let us consider again the case of Instructor A. First, it was quite a shock to him to discover after the first year survey that at the end of the semester, only 7 % more of his students rejected the design argument than at the beginning. Nothing said in class or written on examination papers led him to expect this result. Also, nothing said or written during the year he used d'Holbach's essay gave him a clue that the survey results for the second year would be different from the first. In short, without the survey he would have had no idea of the effect that text and discussion had on the class. Further, he believed, as do all reputable philosophers, that the argument from design is *not a proof* for God's existence. Certainly he did not wish merely to persuade a number of students to agree with him. He wanted them to consider the concept of proof as it is used in the sciences, and then to see that the design argument does not have the characteristics a proof should have. He felt that something is amiss if students on examination papers seem to know what is wrong with the design argument and yet continue to believe it is a proof for God's existence. Of course, he will never know from a survey alone whether students who rejected the design argument at the semester's end did so for the right reasons, but without the survey he would never have known whether they rejected the argu-

ment at all. Taking into account that he was teaching freshman students, A decided that the survey results outweighed the fact that, from a sophisticated philosophical standpoint, Hume's *Dialogues* are more profound than d'Holbach's essay.

Before discussing other significant shifts in student opinion, it would be *a propos* to state how we are using "significant". For the sort of "qualitative" study we were doing, we felt that any statistical analyses more elaborate than a chi-square test of significance were unnecessary. We employed the standard chi-square test used for gauging the significance of so-called "correlated proportions". Initially we saw no *a priori* reason for accepting .05 as the significance level upon which to base our decisions as to whether an opinion change was significant, for we had no way of estimating how important it was for us not to err in rejecting the null hypothesis. We saw no reason for not accepting a result at the .1 or even the .2 level. As it turned out, however, almost all our results proved to be either highly significant or highly insignificant, and we were spared too many "close decisions". Thus when we report a result as significant, we mean significant at a level of .05 or smaller. A serious defect in our experimental design was that we did not pick out a sample of freshman students who were not taking philosophy and use it as a control group. We assumed, without sufficient warrant, that when a shift in opinion was significant, it reflected the effect of a philosophy course. With regard to certain items pertaining to proofs for the existence of God, we did in a sense have a control group because Instructor C did not discuss these matters.

One item on the survey read: "Do you believe that there is a God or do you believe that God does not exist?"

- a. I am absolutely sure that God exists.
- b. I think that God exists, but I could be wrong.
- c. Maybe God exists; maybe he doesn't. I am not sure about this matter, one way or the other.
- d. I think that God does not exist, but I could be wrong.
- e. I am absolutely sure that God does not exist."

In examining the following table of results, remember* that Instructor C did not discuss in any way the problem of the existence of God. The students of Instructors A and B read and discussed Anselm's ontological argument and the five arguments of Aquinas in addition to d'Holbach's essay.

TABLE III

	Instructor A		Instructor B		Instructor C	
	<i>Before</i>	<i>After</i>	<i>Before</i>	<i>After</i>	<i>Before</i>	<i>After</i>
a.	57 %	35 %	54 %	27 %	45 %	28 %
b.	18	35	26	36	31	43
c.	18	23	13	24	15	13
d.	6	6	4	10	9	9
e.	1	1	3	3	0	3

Combining categories, we may call students who selected (a) or (b) "believers," those who selected (c) "agnostics," those who selected (d) or (e) "atheists". Although there was a significant change among the believers in all sections of "strength of belief", only in section B was there a surprising and significant *change* of belief, an increase of 17 % in the students who became either agnostic or atheist.

For a number of items, we subdivided each section first according to religion of student (Protestant, Catholic, Jew). Then we partitioned each of these groups into subclasses: believer, agnostic-atheist (combining thereby categories (a) with (b), and (c) with (d) and (e)). The first item examined after the partitioning read: "There is no conclusive proof that God exists." Students again could strongly agree or disagree or tend to agree or disagree. In section A, the percentage of Protestants who believed in God while holding that there was no proof for His existence significantly increased. Also, the number of Protestants who believed in God and thought there was a proof for His existence significantly decreased. One might have thought that the more a student became skeptical of the proofs for God's existence, the greater would be the tendency to disbelieve in His existence. This was not the case. Among Catholic and Jewish students, there were no significant changes.

In section B, despite the fact that there was a significant increase in the number of agnostic-atheists produced (see Table III), there was nonetheless among the Protestants a significant increase in the number of students who rejected all proofs yet still believed that God existed, and a significant decrease in the number of those who accepted some proof and believed in His existence. Again, there were no significant changes among Catholics and Jews. In section C, where these matters were not at all discussed, there were no significant changes.

We examined in the same fashion the item concerning the argument from design (see page 143). One would expect the results, if students are

at all consistent, to be somewhat similar to those for the item just discussed. This turned out to be the case. In section A, the number of Protestant believers who accepted the design argument decreased as the number of Protestants believers who rejected it increased. Again, there was no significant shift among Catholic and Jewish students in all sections, and no significant change in section C. In section B, although the Protestant believers who accepted the design argument decreased, the number of Protestant believers who rejected it did *not* increase at the end of the term. We advanced the following hypothesis to explain this curious result. There were a number of students who, at the end of term, not only rejected the argument from design, but also changed from being believers to being agnostics or atheists. This hypothesis was confirmed by examining individually the returns of all those students who at the beginning of term were Protestant believers and at the end were "Protestant" agnostics or "Protestant" atheists. (These students still classified themselves as Protestant.) Almost invariably, these were students who initially accepted the design argument and later rejected it. Probably, for such students, the argument from design was an important article of faith.

In considering all survey items, it was possible to calculate what could be called "the opinion-shift average" for each section. No student changed his opinion on less than 2 items or on more than 19 items. In each section the opinion-shift average was almost the same — about 10 (of possible 40). The "average student," then, changed his opinion on about 25 % of the items offered in the questionnaire.

We then selected two groups of students: those who changed their opinions on 15 or more items and those who changed their opinions on 6 or less items. There was no significant difference between the two groups on the basis of any of the characteristics we could examine by means of the data at our disposal. We had information on mid-semester grades, age, sex, religion, political preferences, fraternal affiliations, and intention to take another philosophy course. In no way were these two groups distinguishable from each other or from the rest of the population from which they were selected. Our findings, or lack of them, were somewhat amusing to us because, being victims of our prejudices, we had vague expectations that women would change their minds more than men, and Democrats more than Republicans.

* * * * *

The results we have discussed are perhaps of more sociological and psychological concern than of philosophical or pedagogical interest. As a final issue we wish to discuss a matter quite germane to the teaching of philosophy — the clarification of belief. Very often a student takes a philosophical position without really understanding that position. One objective of a good introductory philosophy course should be to make the student examine the consequences of his philosophical opinions. In this way, a student gradually becomes aware of the full meaning of a philosophical viewpoint; he may even begin to grasp what it means to take a philosophical position. Opinion surveys can at best provide only a rough gauge of the extent to which a student clarifies his beliefs. An hour's private conversation with a student would provide an instructor with much more information on this score than a whole battery of questionnaires. What a survey can do, however, is to provide information on the consistency of belief.

Suppose that a student assents to a questionnaire item to a proposition P . Suppose that on another item the student is asked whether he assents to not- P . Probably just a small number of students will be so flagrantly inconsistent as to assent to both P and not- P . Thus, if one item reads "Nothing can really be known for certain" and another reads "It is false that nothing can really be known for certain", very few students will agree to both propositions. But suppose one item reads "It is absolutely certain that $3 \times 3 = 9$." We found that many students will say both that nothing can really be known for certain and that it is absolutely certain that $3 \times 3 = 9$. The actual figures were: 55 % of the students surveyed at beginning of term tended to agree or strongly agreed that nothing could be known for certain; 83 % believed it certain that $3 \times 3 = 9$; 25 % believed both that nothing could be known for certain and that it was certain that $3 \times 3 = 9$.

We also found that 50 % of the students surveyed believed at beginning of term both that all values are relative and that "although people may often disagree about what is right and what is wrong, basically certain things are right and other things are wrong no matter what people think about them." No instructor discussed these matters during the semester, and no increase in consistency was noted at the end of the semester.

What accounts for such phenomena? We believe that students often adopt a certain general philosophical position because they think it sophisticated to do so. A set of properly designed questions will show, however, that many students do not really know what they are assent-

ing to when they take such positions. If a student assents to a proposition, but does not accept its logical consequences, he certainly cannot be said to understand the original proposition. If a student is induced by a course in philosophy to begin a self-examination of his beliefs, the effect of such examination should manifest itself by an increase in belief consistency.

As stated earlier, we were able to pair the initial and final responses on each item for each student. It was often possible to determine whether a given student became more or less consistent during the semester. We were also interested in comparing the sections to see if changes in belief consistency varied from section to section. Each instructor discussed epistemological problems and each section read Descartes' first two *Meditations* and part of section II, Part I of Hume's *Enquiry*. Thus, students should have become more aware of what it meant to be a skeptic or to say that nothing could really be known for certain. The survey items about the problem of knowledge read:

- (1) Nothing can really be known for certain.
- (2) It is absolutely certain that 3 multiplied by 3 is 9.
- (3) It is absolutely certain that if I am not wearing an asbestos glove or any other protective material and stick my hand into a flame for one minute, I will burn my hand.

(These items did not occur together in the questionnaire).

It is obvious that a student cannot consistently assert all three propositions, but can consistently deny all three. In testing for consistency, one must choose an item or set of items such that anyone responding in a certain way to these items must also, to be consistent, respond in a unique way to other items. With regard to the above items, those students who agree on both initial and final survey to (1) will form a proper test set, for such students, to be consistent, must disagree on both surveys with (2) and (3). Also, those students who assent to (2) on both surveys must, to be consistent, also disagree with (1). Finally, students who assent to (3) on both surveys, must, to be consistent, also disagree with (1).

We examined each of the three test sets mentioned. We found that they varied in size, that students were much more inconsistent in one test set than in the others, and that there was an inexplicable but significant difference in the initial belief consistency of the three sections of philosophy surveyed.

Let us first consider the class of students who agree on the initial and final surveys with (2) — that it was absolutely certain that $3 \times 3 = 9$.

This set formed 79 % of section A, 72 % of B, 73 % of C. Now a student who agreed with (2) and disagreed with (1) on both surveys was classified as consistent on both surveys. A student in the test set who initially agreed with (1), but later disagreed was classified as inconsistent on the first and consistent on the final survey, and so on. We used the following notation to indicate the four possible cases: "C—C" for consistent on both; "C—I" for consistent on the initial survey, inconsistent on the final survey; "I—C" for inconsistent on the initial survey, consistent on the final survey; "I—I" for inconsistent on both surveys. The next table gives the percentage of students in each of the above categories for each of the three philosophy sections. Remember that these percentages represent not percentages of the total sections, but percentages of the test sets.

TABLE IV

	I—I	I—C	C—I	C—C
Section A	17.5 %	24.5 %	12 %	46 %
Section B	11	31.5	7.5	50
Section C	22.5	47	2	28.5

It may facilitate matters to interpret one of the entries in Table IV. The entry in column 2, row 2 indicates that of those students in Section B who thought it absolutely certain at the beginning and end of the semester that $3 \times 3 = 9$, 31.5 % thought at the beginning of term that nothing could be known for certain, but no longer thought so by the end of the term. Combining entries in columns 3 and 4 gives the percentage of students in the test group who were consistent at the beginning of the semester. As can be seen, there was a great difference in initial belief consistency for sections C and A; in C, 30.5 % of the test group were initially consistent while in A, 58 % of the test group were initially consistent. Subtracting the entries in column 3 from those in column 2 gives the percentage increase in consistency for each section. There was a 12.5 % increase in A, a 24 % increase in B, a 45 % increase in C. A chi-square test shows the increase in section A to be insignificant.

Another test group can be formed by first selecting those students who agreed at the beginning and end of term with item (3) and then checking to see whether they agreed or disagreed with item (1). 62.5 % of the students in section A, 51.5 % in B, and 51 % in C were in this test group. The next table gives the results.

TABLE V

	I—I	I—C	C—I	C—C
Section A	20 %	29 %	6.5 %	44.5 %
Section B	8	28	11	53
Section C	27	36.5	5	32

Again there was great sectional variation in initial consistency. In A, 51 % of the test group were consistent at beginning of term; in B, 64 %; in C, 37 %. There was a 22.5 % increase in consistency in A, a 17 % increase in B, a 31.5 % increase in C. A chi-square test showed the increase in B to be insignificant.

A third test group was formed by selecting those students who agreed with (1) at the beginning and end of term, and then checking whether they agreed or disagreed with (2) and (3). This test group was rather small; it comprised 19 % of A, 17 % of B, 28.5 % of C. Further, most students in this group were inconsistent at both the beginning and end of the semester with regard to both items (2) and (3), and there was no significant increase in the percentage of students in this group who became consistent at the end of the semester. In the main, students in this test group ostensibly maintained that nothing could really be known with certainty. However, the test results suggested that they never examined the basis of their skepticism, for they blithely held throughout the semester such opinions as "It is absolutely certain that $3 \times 3 = 9$ ". One might suppose that most students in this test group were poor students, but their mid-term grades showed otherwise. In fact, a comparison of students who were in any of the four "consistency" categories (I—I, I—C, C—I, C—C) with students in the other categories, for all test groups and all philosophy sections, showed no significant difference in mid-term grades.

A number of conclusions might be drawn — that something was wrong with our questionnaire, that something was wrong in the assignment of mid-term grades, or that the responses to the questionnaire gave information about the extent to which students engaged in self-examination of their philosophical opinions and beliefs, information which couldn't be obtained by reading examination papers. We prefer the last alternative.

In looking over the percentage increase in consistency scores, we found a positive correlation between size of increase and number of students who were initially inconsistent. For example, given two sec-

tions in which there were significant increases, if in the first section there were more students who were initially inconsistent than in the second section, there was a greater likelihood that the percentage increase in consistency would be greater in the first section than in the second. We also found great variations in initial consistency of the different sections. It is necessary to take these facts into account if one is interested in comparing more realistically the effectiveness of different teachers, texts, or teaching techniques.

We have attempted this more realistic appraisal in the following manner. Once it was determined that Instructors B and C had significant increases in consistency according to the data of Table IV, and Instructors A and C according to Table V, we proceeded to compare the groups of students who became consistent after taking the course with the total number of initially inconsistent students in each relevant section. In Tables IV and V this new index is arrived at by taking the entry in column 2 and dividing this figure by the sum of the entries in columns 1 and 2. Thus, although there was a 24 % increase in consistency in B and a 45 % increase in C in the first test set (see Table IV), the percentage of inconsistent students in B who became consistent was 74. In C, this percentage was 68. In the second test set (see Table V), the percentage of inconsistent students in Section A who became consistent was 59; in Section C, it was 57.

We have been suggesting that consistency constitutes a means of measuring the understanding of a philosophical idea. Where students enter a course espousing, explicitly or implicitly, a general idea without being aware of its consequences, one of the aims of the course should be to produce such awareness. Our inquiry must be viewed in light of this aim.

¹ The difference between (a) and (b) or between (c) and (d) may be considered a difference in "strength of belief". Often a student's belief changed in strength after he studied philosophy, but his essential position did not change. Therefore, although a student might initially check (a) and later check (b), we did not consider him to have changed his belief unless he went from (a) or (b) to (c) or (d) or vice-versa. Primarily we were interested in change of belief rather than change in strength of belief. Originally we intended to try to scale philosophical beliefs along various continua, and for this job it would have been helpful to know how strongly a student held a particular belief. However, we never got around to devising such scales. Also, we felt that if we increased the number of options for each item, we would produce an increase in the number of students who committed themselves on a particular item.

STATEMENTS, COMPONENTS AND EXTENSIONALITY

by

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The philosophically important questions concerning what can be deduced from a given statement, of what would constitute a correct analysis or translation of it, of whether to say that P is to say that Q, and others, can be clearly formulated and possibly answered only on the basis of prior clear formulations of what is to be meant by "statement component of a statement". This paper takes up these questions in the context of a discussion of a certain formulation of the so-called Extensionality Thesis. It attempts to show the hidden complexity of this thesis (and those questions) by indicating the consequences for it of several more or less plausible interpretations of the notions of *meaningful statement*, and *statement component*.

A number of disputed issues in what is sometimes called "philosophical analysis", as well as in semantics and logic, involve in their formulations the term "component" or some similar expression, such as "constituent"; or involve statements to the effect that certain expressions are *contained in*, or *enter into*, or *are involved in*, or *assert* some others. Some examples: that form of the so-called extensionality thesis, according to which all compound statements are truth functional with respect to their component statements; the view that in a valid argument nothing may be contained in the conclusion that is not contained among the premises; the view that the meaning of compound statements can be explained in terms of, reduced to, the meaning of the component statements. So too, the question whether a translation of a poem is possible and, if so, correct, can be answered if at all only by reference to what it contains, or to what is involved in or asserted by it.

Now I believe that there is no unanimity regarding the sense or senses to be attached to these terms; but even if there were it would not necessarily follow that some different formulation would not turn out to be more revealing of things of prime importance in

the understanding of language. In any case it appears that there are a number of more or less plausible interpretations of the above, and similar, expressions, and accordingly one may expect to get different answers to questions involving them as one varies their sense. In what follows I wish to examine certain of these formulations in order if possible to throw light on some of these debated issues. As a thorough canvassing of all the main senses attachable to these several terms would be an excessively long and difficult task, I shall content myself here with an attempt to examine some of them, in the context of an appraisal of the so-called extensionality thesis, in terms of certain possibly plausible interpretations of the notion of *statement component* of compound statements, hereafter referred to by "component".

It is obvious that the notion of component (statement) cannot be handled in isolation, for to be a component is to be a statement and a component of one. We must therefore deal with the problem of components (in this sense) in conjunction with the problem of what is to be meant by "statement". The existence of what have been called "pseudo-statements", however conceived, is *prima facie* evidence that not every expression that looks like a statement really is one. For example, the grammatical criteria, being essentially geometrical in nature, if they are to be taken seriously, can be treated only as expressing empirical generalizations to the effect that to be of a certain form (well-formed) is to be a genuine statement. At any rate, this will appear plausible to those, like the present writer, who require of genuine statements that they be either true or false. Accordingly, if we understand by "statement" a sentence that is either true or false, and if by "is true" we understand a relation holding between statement and fact, state of affairs, or however we wish to designate what we find in the world or in ourselves — that is, if we treat *is true* as a semantic relation — then we shall require of a genuine, i.e. meaningful, statement something more than that it be well-formed, that is, in some sense grammatically correct. In what follows this is taken for granted.

How then are we to understand "statement"? We shall here for the sake of definiteness and by way of example restrict ourselves to formulations based on the so-called *verifiability principle*. We shall assume, what is of course debatable, that to be a genuine, cognitively meaningful¹ statement a sentence will have to be Verifiable, in the sense that a sentence is Verifiable if and only if it is either verifiable or falsifiable. "Verifiable" with a capital "V" is used here in a generic sense, as when we speak of women being a part of mankind, and treat falsehood

as a truth-value of statements. Therefore it does not follow, according to this view, that a statement is verifiable if it is Verifiable, any more than that its truth-value is, necessarily, truth. In the sense made use of here, to be verifiable, with a small "v", is to be a statement for which the user (sender or receiver) can specify at least one *ascertainable* condition such that if it holds, the statement, *as he understands it*, must be true; while to be falsifiable a statement must be of such a character that its user can specify at least one ascertainable condition such that if it fails to hold, the statement, as he uses it, will of necessity be false. Such conditions I call "truth-conditions". Expressed somewhat otherwise, a statement is verifiable if and only if its user(s) can specify an ascertainable sufficient truth-condition (stc) for it, and falsifiable if and only if an ascertainable necessary truth-condition (ntc) is specifiable for it, and, consequently, meaningful² if and only if one or the other can be done.

In the above, by "ascertainable" I understand a property akin to *observable*, but not identical with it, at least in the narrow sense of "observable". Thus, that water quenches thirst is in the intended sense ascertainable; that there are (or are not) seven prime numbers between 1 and 11 is also ascertainable. Likewise, that Caesar crossed the Rubicon is, in the timeless sense of 'is', ascertainable (i.e. one could have observed the crossing, or the non-crossing, by having been suitably positioned in time and space), as it is that the world will be destroyed by an atomic explosion in the year 2000 A.D. In contrast one would be inclined to say (I do not want to be dogmatic here, since there are no limits to the meanings that can arbitrarily be assigned to any given expression) that God forgives repentent sinners is not, in the sense just exhibited, ascertainable. Of course if "God forgives..." be interpreted in some figurative sense, then what it asserts may be ascertainable; for example, if one interprets it to be synonymous with "Repentent sinners gain peace of mind", adopting the pious convention that everything that happens is to be understood as in some sense coming from God. So too, in the more or less literal sense we should, I suppose, say that the infinite divisibility of physical space is not ascertainable, that man has an immortal soul is not ascertainable, and so on. I would stress here that to establish the ascertainability of a condition is tantamount to establishing what we should have to *find* or *not find* to determine the truth of a statement affirming or denying the condition in question. In place of "ascertainable" one might say "determinable" or, speaking metalinguistically,

"testable". The circularity with which these attempts at explanation may be charged is innocent provided one knows how to break into or out of the circle, i.e. already understands some member of the circle. One can hardly be sure that unknown readers have reached this understanding after a few examples; but it is necessary to stop somewhere, and for want of knowing a better place I stop here.

The restriction to *ascertainable* conditions is required to cope with objections like the following:

"There would be in your sense no meaningless statements, since every statement satisfies your requirement. For it is both necessary and sufficient for the truth of any statement S that S^3 . Thus, just as "Water quenches thirst" is true if and only if water quenches thirst, so "God forgives repentent sinners" is true if and only if God forgives repentent sinners. And the same holds for any statement whatever. Hence, if to be a genuine, cognitively meaningful statement is to be one for which either necessary or sufficient truth-conditions can be specified, "God forgives repentent sinners" is quite as meaningful as "Water quenches thirst"."

By the restriction of truth-conditions to ascertainable conditions (a restriction I myself consider pleonastic) one may hope that such rebuttals as the one given above lose much of their impact. Whether they do or not, perhaps the reader can more easily comprehend what follows, in which the restriction I have attempted to make clear plays an essential role.

I shall now try to lay down something approximating⁴ to satisfactory definitions of the key semantic concepts underlying this discussion. I say "approximating", for I must confess that I am not wholly satisfied by what follows. But it will serve to express in somewhat more formal and precise language the ideas I wish to use. Each attempt at definition will be put in the form of an equivalence, although strictly speaking one wants something stronger than equivalence, or even than logical equivalence. I should as a matter of fact suppose that an adequate definition would require that definiendum and definiens be at least synonymous terms, and that analysandum and analysans be synonymous statements. But I shall be content with equivalence, at least so far as the major connective in these formulations goes. However, it will be necessary to use a stronger relation than that of the material conditional, symbolized by " \rightarrow ", in the flanking expressions. I shall use the symbol " $e\rightarrow$ " for this stronger relation, which I call "entails". This is stronger than the usual relation that goes by

this name, since I understand it in such a sense that, when used on the metalanguage level, it may be flanked only by names of contingent, non-analytic expressions. In this manner the paradoxes of necessary implication or (usual) entailment will, I hope, be avoided. If we designate this weaker form of entailment by the symbol " $n \rightarrow$ ", we shall understand $P \rightarrow Q$ to be true only if $P \rightarrow Q$ and both P and Q are contingent.

This restriction is meant to take account of the basic insight, or assumption, that truth-conditions are facts or states of affairs corresponding to contingent statements only. The impossibilities and trivialities expressed (if that be the correct term) by analytic statements do not qualify as truth-conditions as these are here conceived.

In the preceding paragraph I said that " \rightarrow " may be flanked only by names of contingent statements when occurring in a metalinguistic context. When understood (as it sometimes is) as designating a relation holding between conditions, not expressions, " \rightarrow " obviously requires to be flanked by noun-clauses, the constituent statements of which may be contingent only, to satisfy the requirement that states of affairs or truth-conditions be neither necessary nor impossible, whatever that would be.

Definitions :

1. That X is a sufficient truth-condition (stc) of $S \leftrightarrow$ that $X \rightarrow$ that S is true.

2. That X is a necessary truth-condition (ntc) of $S \leftrightarrow$ that $\sim X \rightarrow$ that S is false.

3. S is verifiable $\leftrightarrow S$ has at least one (ascertainable) stc.

4. S is falsifiable $\leftrightarrow S$ has at least one (ascertainable) ntc.

5. S is a meaningful statement $\leftrightarrow S$ is either verifiable or falsifiable.

6. That X is the meaning of $S \leftrightarrow$ that X is the nstc (necessary and sufficient truth-condition) of S .

7. That X is the same nstc as that $Y \rightarrow$ (that $X \rightarrow$ that Y). (that $Y \rightarrow$ that X)⁵.

8. S and S' are synonymous $\leftrightarrow S$ and S' have identical nstc's.

In 1 and 2 above, and in similar contexts, X is in any concrete case to be replaced by a declarative sentence and S by the name of such a sentence. Thus we would have, for example, in 1: That Jones is a father is a sufficient truth-condition of "Jones is a parent" \leftrightarrow that Jones is a father \rightarrow that "Jones is a parent" is true.

As I am not here concerned primarily with a defence of the semantic notions used I do not feel obliged to discuss them at any great length. Objections can be found to any formulation if one looks hard enough. I am concerned only with making clear certain consequences of the meaning of "genuine statement" for the problem of components, and related issues. However, I shall briefly point out some consequences of the above formulations, as well as attempt to remove one or two difficulties.

The first difficulty is, of course, how one ascertains that a condition satisfies the requirement for being either a necessary or a sufficient truth-condition of a given statement. How do I know, that is, that Jones's being a father is a sufficient truth-condition for "Jones is a parent"? It would seem that first I must know whether this condition entails the truth of the statement, which presumably I can know only if I already know the meaning of the statement. Thus it might be, and has been, argued that the concept of truth-condition does not explain meaning or meaningfulness, but presupposes them. Concerning this objection it must be pointed out that we have here no question of priorities, temporal or logical. It is not the case that *first* we ascertain the truth-conditions and *then* clarify the meaning or meaningfulness, nor is the contrary the case. It is simply *one and the same thing*, according to our definition, to know that a statement has either a stc or a ntc and to know that it is meaningful; and it is one and the same thing to know the ntc of a statement and to know its meaning in the sense of *what* it means. *If* we know the meaning of a statement, in the sense of "meaning" here under discussion, then we *do* know its ntc; and if we know that it is meaningful, then we know at least one stc or one ntc of it — these consequences flow from the definitions.

The decisive question is whether we ever do know one or the other. But this is easy to determine. If anyone doubts that I know a sufficient truth-condition for "this object has one of the rainbow colors", I can eliminate his doubt by specifying at least seven; and if he wonders how I can be sure that they are all sufficient truth-conditions, I can inform him that what I mean by "has a rainbow color" is simply nothing more nor less than: has the color violet or the color indigo . . . or the color red. — Just as it is sufficient to establish that I can swim by swimming, so it is sufficient to show that I can specify a stc of a given statement by doing so. And it is clearly not to the point for anyone to deny that I *can* under these conditions, or to tell me that

someone else might mean or does mean something quite different by the sentence under discussion. This is immaterial, since in explaining what I mean I make no claims whatever as to what if anything someone else may mean by the same sequence of words. That is his problem — although it does not of course follow that he does mean something else. Agreement in meaning is by no means hard to come by, provided we know what we are seeking.

If anyone verified or falsified a statement, then he knew it to be, in the above sense, meaningful, since he ascertained the presence of a sufficient or absence of a necessary truth-condition for it; and if anyone ever ascertained a stc (for a statement) whose non-existence would have made the statement false, then (in my sense) he knew *the* meaning of that statement. I do not think one runs any serious risk of being dogmatic, in an objectionable sense, should he claim that both of these conditions have on occasion been satisfied — and I must here frankly confess to a certain scepticism concerning the often-heard view that it is impossible to verify or falsify any statement.

In the preceding paragraphs when I speak of *the* meaning of a statement I do not wish to be understood as denying the obvious facts of ambiguity; on the contrary I claim to be explaining them. For if, as is often the case, different people use (or even one and the same person uses) a given *sentence* with different meanings, i.e. different necessary and sufficient truth-conditions, it doesn't follow that the uniqueness implied by the expression "the meaning" is denied. If one and the same *sentence* is differently interpreted by virtue of the assignment or determination of different nstc's for it, it follows that a number of different statements are involved, and precisely as many different statements as there are different nstc's attached to the sentence. Its meaning is not something externally related to the *statement*, as it is to the sentence, but integral to it. Consequently it is a contradiction here to speak of a statement (but not of a sentence) as having more than one meaning, although it may well have less than one; namely if, as we shall see is possible, it has stc's, but no ntc's, or the contrary; or even none, if pseudo-statements are statements.

I may briefly point out that the formulations given above remove some at least of the familiar objections to the improperly so-called verifiability *theory* of meaning. In particular, according to this version of the principle, analytic statements as well as nomological statements are meaningful. It requires for a statement to be meaningful only that it be ascertainable whether it be true or false. I should hold that less

than this no satisfactory definition of "meaningful" can offer; more would appear to be of no advantage.⁶ But however that may be, we leave the topic of statement meaning to turn to that of components, with the final observation that the formulations given above do not in any way settle any particular semantic question, such as whether statements of psychology, theology, metaphysics, or any other domain of discourse, are meaningful, and if so what if anything they do mean. Thus, for example, it does not follow from the foregoing definitions that statements about other minds are (of course for certain persons) meaningless, although they may be; — namely, if their users cannot specify either necessary or sufficient ascertainable truth-conditions for them, as *they* understand them. Whether they can or cannot is no concern of the formulation, which will not *of itself* settle any such question. The question can be settled if at all only by the speaker himself, who alone can tell us what if anything in the realm of ascertainable fact either entails or is entailed by the truth of his statement as *he* understands it — for his understanding determines in what entailment relations it stands.

To analyze or make clear the meaning of any statement is thus in terms of the above conceptions simply to specify the necessary and sufficient conditions for its truth. Since this process can sometimes be carried only part way, we can sometimes specify sufficient but not necessary or necessary but not sufficient truth-conditions, we are prepared to learn that we can sometimes make only partial analyses of meaning. A sufficient condition that a statement be meaningful is thus that it be partially analyzable. This process of analysis will play an essential role in all that follows. A few examples may be of use in making the notion clear:

I. "Jones is an uncle."

In one of the senses of "uncle" this statement will be true if and only if the following three (not irreducible) conditions are satisfied: (1) that Jones is a male, (2) that Jones has/had/ a brother, or Jones has/had/ a sister, and (3) that a brother of Jones has/had/ a child, or a sister of Jones has/had/ a child. Separately, each of these three conditions constitutes a necessary condition for the truth of the statement, and collectively they constitute a sufficient condition for the truth of the statement. Therefore in our sense the statement means that these three conditions are satisfied. The statement is fully analyzable — hence, *bi-valent*.

II. "Jones is all-knowing."

It is clear that a necessary condition for the truth of this statement will be that Jones be able to answer a given question, say Q_1 . But it is perhaps equally clear that there is no end to the questions he would have to be able to answer if the truth of the statement were to follow; hence the normal interpretation of the statement would make it one for which we could not list any ascertainable sufficient truth-condition. It will be evident, of course, that the same is true of all analytically false statements. They are only partially analyzable — thus, *uni-valent*.

III. "There exists in addition to the earth at least one other body that supports life."

This statement will of course be true if Mars supports life, but on the assumption of the infinite extent of space it will be impossible to find a necessary ascertainable condition for it, since no matter how many such bodies might be found exhibiting no life, it would not follow that the statement would of necessity then be false. Tautologies will be similar in this respect.

IV. "War is inevitable."

In one (intended) sense at least this statement can be provided neither with necessary nor with sufficient truth-conditions, and would thus qualify as in our sense cognitively meaningless. At least (according to it) if war does occur, it does not follow that it was inevitable; nor if it fails to occur up to any given ascertainable moment, does it follow that it is not inevitable. I do not claim, however, that there is *no* sense in which this same sentence would be a meaningful statement, since the contrary is evident enough.

II.

We may now turn directly to the problem of the nature of components or, what amounts to the same thing, to a consideration of some of the more interesting formulations or interpretations of the meaning of "component". The first and probably most widely accepted interpretation is the following:

I (1): S is a component of S' \leftrightarrow S is a part of S'

Here "part" is understood in the obvious geometrical sense. Thus S is a part of the expression $S.P$, being identical with that segment lying to the left of the dot. Similarly, P is a part of "A believes that P ", being identical with the segment of the expression lying to the right of "that". In this sense of "component" both P and Q are com-

ponents of 1) $P.Q$, 2) $P \vee Q$, 3) $P \rightarrow Q$. In the above examples both P and Q will be, in this sense, component statements of the expressions, provided they are statements; and the whole expressions of which they are components will be compound statements, provided they are statements.

It is obvious that not every part of a statement is a statement. Thus in "A believes that the earth is a sphere", "A believes that" is not a component statement, not being a statement at all. On the other hand, it is clear that "the earth is a sphere" is a component statement of the original expression, which itself would appear to be a compound statement, being a statement and having a component statement, in the sense of part.

In this sense of "component" "U is an uncle of B", if it has any component statement, has "U is an uncle" and "U is" as such. But we may doubt whether "U is" is a statement at all; and some will question whether "U is an uncle" is a component statement of "U is an uncle of B", holding that the predicate properly conceived is "is-an-uncle-of B", which does not contain "is an uncle" as a part. Such ambiguities as this are among the reasons why one hesitates to accept the identity of part and component. In any case it is worth while to consider other possible interpretations.

Since all the conditions necessary for the truth of a statement together comprise a sufficient condition for its truth, and since we identify the meaning of a statement with the necessary and sufficient condition for its truth, we may usefully think of its components, in at least one sense, as comprised of statements describing those necessary conditions which taken together give us also a sufficient condition. We therefore make our first revision of our original formulation of the sense of "component" (according to which to be a component is to be a part, and to be a component statement is to be a statement-part of a statement) as follows:

RI(1) (Revised Interpretation One):

S is a component statement of S' $\leftrightarrow (S' \text{ e} \rightarrow S)$

If we examine this conception, we note the following consequences (among others): (1) Every conjunction, and statement synonymous with the conjunction, has as components each of the conjuncts, but, (2) not every disjunction, or statement synonymous with a disjunction, has as components any particular one of the disjuncts. For the truth of a conjunction entails the truth of all its conjuncts, but the truth of a disjunction does not in general entail the truth of any particular disjunct, although at least one must, of course, be true. Thus,

"It is sunny and the wind is blowing" entails both "It is sunny" and also "the wind is blowing", but "It is sunny or the wind is blowing" does not entail either "It is sunny" or "the wind is blowing", although of course it does entail "It is sunny or the wind is blowing", since every statement does, so to speak, trivially entail itself. Similarly, $P \rightarrow Q$, under the standard interpretation: $P \rightarrow Q = \sim P \vee Q$, does not in general entail either P or Q (strictly, the statements they represent), for neither describes a necessary condition for its truth.

This view that statements are components of compound statements if and only if they describe necessary conditions for the truth of the compound is not unknown; at least it appears to be the view underlying certain formulations found in the literature. Although he does not use the term "components" in *this* sense, it would seem that Gilbert Ryle has some such *conception* in mind, if perhaps only implicitly, (see his essay, "'If', 'So' and 'Because'," *Philosophical Analysis*, p. 334; Cornell University Press, Ithaca, 1950, ed. Max Black) when he says, for example: (1) 'The statement "if p , then q " does not incorporate the statements " p " and " q ", as these statements are certainly incorporated in such dicta as " p and q ", " q , because p ", " p , so q " and " p , although q ".' (2) 'In saying "if p then q ", I am not stating " p " or " q " or in any way committing myself to the truth of " p " or " q "; I am stating or asserting something, but I am not stating or asserting them. Neither the statement " p " nor the statement " q " enters into the statement "if p , then q ".' (3) '... "if p , then q " is deceptive. For the letters " p " and " q " as they occur here look and sound just like the " p " and " q " that occur in conjunctive statements, inferences and explanations.'

All these remarks can apparently be translated over into our language of components and parts without essential change. For in terms of our present formulation, as we saw, neither p nor q is a component of *if p , then q* , but both p and q are components of *q , because p* ; *p , so q* ; *p , although q* , as well as *p and q* . What Ryle says using the expressions "to be incorporated in", "to enter in", "to be committed to in asserting", holds as well of what in sense RI(1) we mean by "to be a component of". In each case, if I am right, the decisive property is that of expressing a necessary condition for the truth of the statement. At any rate this appears to be a hypothesis in conformity with such facts as lie before us.

However, since we are not defending this view, but only examining it, we need not seek for authoritative use. What is interesting in the

view under discussion is the fact that according to it a statement may be a part of a statement, without being, in our sense, a component of it; and statements may be components of other statements without being parts of them. Thus we saw that although both P and Q are parts of $P \rightarrow Q$, neither is in sense RI(1) a component of it, while "A is a male" is a component of "A is a father" although not a part of it. Evidently neither P nor Q is in general a component of $\sim (P \sim Q)$, nor is $\sim P$ or Q a component of $\sim P \vee Q$.

On the basis of parts that are not components, and components that are not parts we can establish a possibly useful classification of component statements. We distinguish first between: (1) *Virtual* components and (2) *Apparent* components. We shall say:

- (1) S is a virtual component of $S' \leftrightarrow S' \text{ e} \rightarrow S$.
- (2) S is an apparent component of $S' \leftrightarrow S$ is a part of S'

Relative to these we may introduce the notions of *Genuine* and *Pseudo* component:

- (3) S is a genuine component of $S' \leftrightarrow S$ is a virtual component of S'
- (4) S is a pseudo-component of $S' \leftrightarrow S$ is an apparent component of S' . S is not a virtual component of S'

Diagrammatically:

		Components	
		Genuine	Pseudo
Apparent	
	Virtual	+++++	

In accordance with this classification, all those statements listed above as being parts of certain compounds, but not entailed by them are now to be called, relative to the given sense of "component", "pseudo-components". This may give some comfort to those who are made unhappy by a view that permits a statement to be a part of but not a component of a statement, and still make sense of, say, a view like that of Ryle's quoted above (as we have interpreted it).

An interesting consequence of this revised interpretation of "component" is that any statement will have as components all contingent disjunctions of which it is a disjunct, hence an indefinite number. For, $P \text{ e} \rightarrow P \vee \text{---}$, whatever be the statements following \vee . But of

course it does not follow that these statements themselves are entailed by P simply by virtue of being parts of entailed statements, for, as we saw, a disjunction does not in general entail any particular one of its disjuncts. P and Q are, in terms of the distinction introduced above, pseudo-components of $P \vee Q$, being apparent, but not virtual.

The interpretation of "component" according to which disjuncts are not components of their disjunctions is in some respects perhaps counter-intuitive. At any rate it is customary to treat disjunctions as being extensional with respect to their disjuncts, often called "components". It will therefore be useful to consider a formulation of the meaning of "component" according to which the disjuncts of a disjunction are components of it. We call this interpretation "Revised Interpretation Two" and symbolize it by "RI(2)".

RI(2): S is a component of $S' \leftrightarrow S \text{ e} \rightarrow S'$

According to this conception, a statement will be a component of another if it describes a sufficient condition for the truth of the latter. Thus, since this-being-red is a sufficient condition for this-being-colored, it follows that "This is red" $\text{e} \rightarrow$ "This is colored", and hence that "This is red" is a component of "This is colored". Since "This is red" is not a part of "This is colored", it must be a virtual component of the latter. In this sense, RI(2), any statement P will be a component of every disjunction of which it is a disjunct. In a conditional statement $P \rightarrow Q$ the antecedent, P , will not be a component, not expressing a sufficient condition for the truth of $P \rightarrow Q$; but Q will be a component, as will $\sim P$, for both of these entail $P \rightarrow Q$. P will thus be a pseudo-component of $P \rightarrow Q$, for while apparent, that is a part, it is not virtual. While $\sim P$ will be a virtual component.

Evidently it follows from RI(2), too, that every statement will have an indefinite number of components if it has one, for " $(P \text{ e} \rightarrow Q)$ " $\text{e} \rightarrow$ " $[(P \text{ ---}) \text{e} \rightarrow Q]$ ", whatever statement be put in place of "---". It would be possible to eliminate this consequence and the analogous one for RI(1), for example, by providing that no component contains superfluous statements, "superfluous" being defined as follows:

Statement T is superfluous in statement S in relation to statement $S' \leftrightarrow$

- (1) T is a part of S
- (2) S is part of S'
- (3) $(S^* \text{ n} \rightarrow S')$. ($S' \text{ n} \rightarrow S^*$), where S^* is the result of deleting T from S in S' .

Whether this would result in a formulation less objectionable than that permitting a component to contain superfluous statements is however, doubtful, and I do not wish to make a recommendation on this point. (I am indebted here and elsewhere to Benson Mates, for criticisms and suggestions).

In the paragraph above it should be noted that a systematic change in the sense of "virtual" has taken place, for what is a virtual, i.e. genuine, component will vary with the given formulation. RI(2) differs both from RI(1) and the original formulation (component = part). Since in RI(2) S is a component of $S' \leftrightarrow S \leftrightarrow S'$, it will be a virtual component of S' provided it entails the latter. It was in a different sense RI(1) that we earlier said that a statement is a genuine component if and only if it is entailed by the compound. This systematic ambiguity is to be kept in mind throughout. Thus, while for RI(1) both P and Q are components of $P.Q$, this is not the case according to RI(2), for neither part of the compound entails it. Thus in this sense RI(2) both P and Q are pseudo components of $P.Q$, for they are apparent, but not virtual.

Given two alternative competing formulations of a concept, the natural effort to decide between or reconcile them leads obviously first to the formulation of an inclusive concept which is their disjunction. Hence we arrive at Revised Interpretation Three:

RI(3): S is a component of $S' \leftrightarrow [(S \leftrightarrow S') \vee (S' \leftrightarrow S)]$

It will not be necessary to point out the obvious consequences. Some will no doubt find them objectionable and take the second path of reconciliation, namely, that of forming a concept covering ground common to the two competitors. Thus we arrive at Revised Interpretation Four:

RI(4): S is a component of $S' \leftrightarrow [(S \leftrightarrow S').(S' \leftrightarrow S)]$

This will hold when and only when S and S' are synonyms, i.e. have identical conditions of truth and falsity — a consequence which many will also find objectionable.

Unfortunately none of our five proposals is altogether satisfactory, if this is to be of such a character as to satisfy all the obvious possible desiderata. Apparently we must reconcile ourselves to formulations a bit odd, and not only, of course, in this connection. Precision itself is a mark of oddity in a world of predominantly rough and confused discourse; and if we seek precision, we must do so often at the expense of familiarity and other cosy qualities. The proper approach will be rather to decide which formulation to adopt in terms of their several

consequences — provided an examination shows any to be superior in this respect to the others. I do not of course rule out the possibility or likelihood that some other formulation not listed would turn out to be more acceptable than any I have given. For example, Benson Mates has suggested that the following would combine some of the advantages of those I have proposed:

S is a component of $S' \leftrightarrow \{(S \text{ is a part of } S'). [(S \rightarrow S') \vee (S \rightarrow \sim S') \vee (\sim S \rightarrow S') \vee (\sim S \rightarrow \sim S')]\}$

On this definition P is a component of $P.Q$ and of $P \vee Q$ and of “A knows that P ”, but not of “A believes that P ”. However, as this essay is of an exploratory and tentative nature I may perhaps be excused from adding to the list of definitions of “component”, and proceed to draw what consequences I can from the materials already presented. Therefore we turn now to consider the problem of extensionality in terms of these several conceptions of component statement.

As indicated earlier, this is done not with the hope of establishing any particular thesis, but in order to throw some light on certain important questions, for example: Is the meaning of every compound statement a function of the meaning of its component statements? Or, for instance: Is the meaning of a causal statement a function of the meaning of its components? Evidently the answer will depend on what its components are, if it has any.

III.

We shall examine four statements whose extensionality may be plausibly denied. Each of the four will be examined in turn to determine: (A) whether it is a genuine statement in terms of the several criteria of being (1) verifiable, (2) falsifiable, (3) verifiable or falsifiable, (4) verifiable and falsifiable; and (B) whether in terms of the several criteria for having components, it has components. Only when a statement shows itself to be genuine and to have components will it be possible or relevant to raise the question of extensionality, will the property *extensionality* be *applicable*, in the sense that it can be significantly affirmed or denied of the statement in question. It will be evident that we shall get many different answers, depending on the particular criteria used; that one and the same sentence will under some interpretations be, at least possibly, extensional, and under some others not. If after this investigation it be our wish to maximize the scope of the extensionality thesis, we may come to some decision

regarding the senses to be attached to "genuine statement" and "component" that will lead to the wished-for results.

Our four statements are:

- I. "All men are rational."
- II. "Keats believed that Cortez discovered the Pacific."
- III. "Socrates died because Socrates drank the hemlock."
- IV. "If I were rich then I'd be happy."

It will be noted that while earlier, discussing meaning, I emphasized criterion (3): S is meaningful $\leftrightarrow S$ is verifiable or falsifiable, I here examine the consequences of adopting in turn each of the four criteria for meaningfulness, listed in Table IA as (1), (2), (3), and (4).

Table IA Genuine Statement

Statement	(1) verifi- fiable	(2) falsi- fiable	(3) v or f	(4) v and f	
All men are rational	—	+	+	—	'+' = 'genuine statement' '—' = 'pseudo-statement'
Keats believed that Cortez discovered the Pacific	+	+	+	+	
Socrates died because Socrates drank the hemlock	—	+	+	—	
If I were rich then I'd be happy	+	+	+	+	

Table IB. Components

sense of 'com- ponent' statement	I(1)	RI(1)	RI(2)	RI(3)	RI(4)	
All men are rational	—	+	—	+	—	'+' = 'has compo- nent statements' '—' = 'does not have component statements'
Keats believed that Cortez discovered the Pacific	+	+	+	+	+	
Socrates died because Socrates drank the hemlock	+	+	—	+	—	
If I were rich then I'd be happy	—	+	+	+	+	

Tables IIA, B, C, D. Combined Tables

IIA All men...

I(1) RI(1) RI(2) RI(3) RI(4)
(a) (b) (c) (d) (e)

(1) v	-	-	-	-	-
	-	+	-	+	-
(2) f	+	+	+	+	+
	-	+	-	+	-
(3) v or f	+	+	+	+	+
	-	+	-	+	-
(4) v and f	-	-	-	-	-
	-	+	-	+	-

IIB Keats believed...

I(1) RI(1) RI(2) RI(3) RI(4)
(a) (b) (c) (d) (e)

+	+	+	+	+
+	+	+	+	+
+	+	+	+	+
+	+	+	+	+
+	+	+	+	+
+	+	+	+	+
+	+	+	+	+
+	+	+	+	+

IIC Socrates died because...

(1) v	-	-	-	-	-
	+	+	-	+	-
(2) f	+	+	+	+	+
	+	+	-	+	-
(3) v or f	+	+	+	+	+
	+	+	-	+	-
(4) v and f	-	-	-	-	-
	+	+	-	+	-

IID If I were...

+	+	+	+	+
+	+	+	+	+
+	+	+	+	+
+	+	+	+	+
+	+	+	+	+
+	+	+	+	+
+	+	+	+	+
+	+	+	+	+

Table IA shows (on the basis of certain defensible interpretations) for each of the statements whether, relative to each of the criteria listed, it is a genuine statement. Table IB indicates for each of the statements whether it has component statements in terms of the five senses of "component" discussed above. Since there is no universal agreement on the meaning (i.e. truth-conditions) of some of these statements, I have in certain cases made perhaps arbitrary decisions; but this need not be of any harm, since the points I wish to make are of a general character not seriously affected by the decisions taken in respect to the particular illustrative statements under discussion. Thus, in Table IA, I treat "Keats believed that..." as if it were both verifiable and falsifiable, although not everyone will accept this, for some would hold that psychological sentences of this kind are not either verifiable or falsifiable, at any rate when the statement is about someone other than the speaker. At least in part these are matters of decision regarding usage, but there is no doubt that there are familiar senses of such psychological sentences according to which they are both

verifiable and falsifiable. In general what follows is to be taken as tentative and hypothetical in character. If, for instance, it were possible to argue convincingly that "I were rich", in "If I were rich then I'd be happy", is, according to a plausible interpretation, a genuine statement, (say verifiable or falsifiable) I should be willing to abandon the decision according to which, under the interpretation I(1) of "component" I make out the statement not to have any components; and so on.

In Tables IIA...IID, which combine Tables IA and IB, I attempt to show, for each of the four statements, the interpretations according to which they are genuine and have components, in terms of the several senses of each given in the tables. The upper left hand triangle of each square contains a "+" or a "-", depending on whether, according to the sense of "genuine statement" involved, the statement is genuine or not, "+" indicating that it is, "-" indicating that it is not. Each lower right triangle of a square contains likewise a "+" or a "-", depending on whether, for the sense of "component" involved, the statement does or does not have components. Thus, for example, in Table IIA the upper left hand square, la, contains two "-" signs, the upper indicating that the statement "All men are rational" is not genuine in the sense that a statement is genuine if and only if it is verifiable, and the lower indicating that the statement has no components in the sense that it has no parts that are statements. Again, in IIA, square 3c, for example, contains an upper "+" and a lower "-". The "+" indicates that the statement is genuine in the sense that to be so is to be verifiable or falsifiable, while "-" indicates that the statement has no components in the sense that it has no sufficient conditions of truth. With these explanations out of the way I now turn to a discussion of extensionality as it relates to the individual statements. I shall first try to determine the *applicability* of the property *extensionality* to each statement under each interpretation of "genuine statement" and "component statement".

I(1): S is a component of $S' \leftrightarrow S$ is a (statement) part of S'

- (1) "All men are rational", having no statement parts, has no component statements; hence, it can be neither extensional nor non-extensional. The property *extensionality* is inapplicable.
- (2) "Keats believed that Cortez discovered the Pacific", having as a statement part "Cortez discovered the Pacific", has a component; hence it might be extensional provided the statement as a whole is genuine. Since we interpret it as both verifiable and

falsifiable, it follows that it is genuine in all senses of the term here involved, and hence *extensionality* is applicable in all interpretations.

- (3) "Socrates died because Socrates drank the hemlock", having statement parts, has components, and hence the property *extensionality* is applicable, provided the statement as a whole is genuine. The statement is certainly falsifiable, hence verifiable or falsifiable; it is therefore genuine in senses (2) and (3). It appears not to be verifiable in terms of the truth-values of its components in sense I(1), and therefore it will not be genuine in senses (1) and (4) unless it turns out to be verifiable in terms of conditions not describable in terms of its statement parts. This possibility we leave open at present, and provisionally conclude that *extensionality* is inapplicable in senses (1) and (4) of "genuine statement".
- (4) "If I were rich then I'd be happy", since it contains no statement parts (neither "I were rich" nor "I'd be happy" are genuine statements, that is, neither is verifiable or falsifiable), has no components; hence *extensionality* is inapplicable, quite apart from the question of whether and in what sense the statement itself is genuine. I assume that it is both verifiable and falsifiable, hence genuine in all four senses — verifiable on the grounds that if after making the assertion I should become rich and at the same time happy, I would consider the statement to have been true, while if I had become rich and not happy I would consider the statement to have been established as false. Perhaps a more adequate analysis would require also for the truth of the statement a true nomological conditional: "I am rich \rightarrow I am happy", i.e. one derivable from a well-established law, hence not true merely on the strength of a false antecedent or a true consequent. The fact that law statements are not strictly verifiable, but at best only falsifiable would make it impossible to establish the original subjunctive conditional as more than highly probable. Under this interpretation then it would be meaningful in senses (2) and (3) and not in senses (1) and (4), with the obvious consequences. But for the sake of simplicity I shall adopt the more shallow interpretation.

RI(1): S is a component of $S' \leftrightarrow S'e \rightarrow S$

- (1) "All men are rational", being falsifiable, has components in sense RI(1); hence *extensionality* is applicable, provided the

statement is genuine. Having conditions of falsity, it will be genuine in senses (2) and (3); but having, as we here assume, no ascertainable sufficient truth-conditions, *extensionality* will not be applicable in senses (1) and (4).

- (2) "Keats believed that Cortez discovered the Pacific" having, as we assume, conditions of falsity, i.e. necessary conditions of truth, has components in sense RI(1); hence *extensionality* is applicable provided the statement is genuine. Since it has conditions both of truth and of falsity, it will be genuine in all four senses.
- (3) "Socrates died because Socrates drank the hemlock", since it would be false if either statement part were false, has components, for it has necessary conditions of truth; accordingly it is genuine in senses (2) and (3). We shall leave open the further question whether it is also meaningful in senses (1) and (4), which would be the case only if it were verifiable. Its truth at least is not a function of the truth of its statement parts, for in that case $P.Q$ would be equivalent to P because Q , which is evidently not the case. We shall return to this problem later.
- (4) "If I were rich then I'd be happy", since we assume it to be both verifiable and falsifiable, has components in sense RI(1). Since it is meaningful in all four senses, according to our assumptions, *extensionality* is applicable under each interpretation of "genuine statement".

RI(2): S is a component of $S' \leftrightarrow S' \text{ e} \rightarrow S$

- (1) "All men are rational", having no ascertainable sufficient conditions for its truth, is without statement-components in sense RI(2). It cannot therefore be extensional or non-extensional; *extensionality* is inapplicable here, regardless of the question whether the statement is genuine or not, and in which sense.
- (2) "Keats believed that Cortez discovered the Pacific", having, on our interpretation, sufficient conditions for its truth, has components; hence, since the statement is assumed to be both verifiable and falsifiable, *extensionality* is applicable in all four cases.
- (3) "Socrates died because Socrates drank the hemlock" we, for the time being, assume has no sufficient truth-conditions, hence has no components in sense RI(2). It cannot therefore be either extensional or non-extensional, for the usual reasons. Later we shall consider an analysis of this statement which might give us some grounds for thinking it possibly quasi-verifiable.

- (4) "If I were rich then I'd be happy", since we treat it as verifiable and falsifiable, has sufficient truth-conditions, and hence has components; *extensionality* is applicable in all four cases.

RI(3): S is a component of $S' \leftrightarrow [(S \rightarrow S') \vee (S' \rightarrow S)]$

Since the answers here depend solely on these already given, it will not be necessary to go through the list again. The evident results are given in tables IIA–D under the appropriate column; viz. RI(3)

RI(4): S is a component of $S' \leftrightarrow [(S \rightarrow S') \cdot (S' \rightarrow S)]$

Same as above; the results are given under column RI(4).

IV.

If we examine the four tables IIA–D, we shall see confirmed our earlier statement that we should expect to get rather different answers to the question of extensionality, depending on the criteria of genuine statement and of component adopted. In Table IIA we see that of the twenty possible cases relative to "All men..." only the following satisfy the condition of applicability of *extensionality*; namely, 2b, 2d, 3b, 3d. In all the other sixteen cases the question is irrelevant, for in them the statement "All men are rational" is either not genuine or has no components.

On the other hand, (Table IIB) under the interpretation given of "Keats believed...", etc., all twenty possibilities are such as to permit the applicability of *extensionality*.

In Table IIC ("Socrates died because..."), on the basis of the given assumptions regarding meaningfulness of the causal sentences in terms of the four criteria, and in terms of the five different senses of "component", we find that only cases 2a, 2b, 2d, and 3a, 3b, and 3d provide for the applicability of *extensionality*; in all others the question cannot be legitimately raised in terms of the assumptions of this analysis.

Similarly, in Table IID ("If I were rich..."), granting the criteria of having components and our sense of "genuine statement", we see that *extensionality* is applicable in but sixteen of the twenty possible cases. As to whether it *holds* in these sixteen, only further investigation would tell — as of course is also true in the possible cases listed in the other tables. We now turn to some of the problems involved in such a determination.

We have stipulated that for the question of extensionality to be legitimately raised regarding a statement (i.e. for *extensionality* to be

applicable) it must be both genuine and have components. This stipulated condition for legitimacy of the question is, however, only a necessary but not a sufficient condition for answering it. That a statement is both genuine and has components in some sense of these terms does not yet suffice to determine whether it is extensional or not, although that it does not satisfy these conditions is sufficient to establish the irrelevancy of the question of extensionality.

Even if a sentence is both a genuine statement and has components, we are not completely enlightened regarding its nature, since, as we have indicated, these terms are ambiguous, and hence, if we may so speak, the properties of being genuine and having components are likewise. That certain sentences satisfy the necessary conditions for the applicability of *extensionality* does not guarantee that they do so by virtue of having a common characteristic. For example in Table IIA, as we saw, "All men are rational" satisfies the necessary conditions in four of the twenty possible cases, but it satisfies them in four different ways. That is, in case (2a) applicability is satisfied by virtue of: (1) the statement being meaningful in the sense of being falsifiable, and (2) having component statements in the sense of having necessary truth-conditions. In (2d), however, it is satisfied by virtue of the statement being: (1) meaningful in the sense of falsifiable, as in (2a); but, (2) by virtue of having components in the sense of having necessary or sufficient truth-conditions, which is not the same as having necessary truth-conditions. Similarly in cases (3b) and (3d) of Table IIA, the conditions of *extensionality* being applicable likewise differ. And the same holds in relevant cases in the other tables where a square is found with two "+" signs.

But this ambiguity is not all. When we come to deal not with the question of the *applicability* of *extensionality* (here taken as truth-functionality), but with whether it does in fact hold in those cases in which it might, further ambiguities arise. We may distinguish three relevant cases:

I: Both truth-values of a given statement are determined solely by the truth value of the components.

II: Only the value *truth* is determined by the truth-values of the components.

III: Only the value *falsehood* is determined by the truth-values of the components.

These distinctions arise because we have recognized sentences (univalent) for which but a single truth-value, sometimes truth, sometimes

falsehood, is determinable, as well as the more usual (bi-valent) sentences for which both are determinable. That is, some statements are only verifiable, others only falsifiable, and others of course both — and still others neither, if we are to call them “statements” at all.

The question then arises whether a given statement is extensional. If it satisfies the above-described requirements for being both genuine and having components, it could possibly be so. But is it? If we require that *both* truth values be determinable by the truth-values of its components, we shall have to deny that it is extensional in those cases where we deal with a statement having but a single determinable truth value. Yet if we weaken the requirement so that it might be extensional (we might call it “quasi-extensional”) also, even if only a single truth-value is determinable by the truth-values of the components, then we shall in some cases answer the question regarding extensionality differently. And of course again differently provided we specify a particular truth-value, say truth, rather than either truth or falsehood. Referring again to our tables, we observe the following facts:

Of the twenty cases dealt with in Table IIA, but four are such that the question of extensionality can legitimately be raised in any of our specified senses; in IIB all twenty are legitimate in this respect; in IIC only six of the twenty are legitimate cases, while in IID, sixteen are legitimate, — making forty-six out of a total of eighty. But when we apply the more rigorous requirements mentioned above, we get a rather different distribution: If to be extensional a statement must have both truth-values determinable, then only nine of the eighty satisfy the necessary conditions for applicability, there being no cases in Table IIA; five in IIB, namely 4a, 4b, 4c, 4d, and 4e; none in IIC, and finally four in IID, namely, 4b, 4c, 4d, and 4e.

If we relax this strongest requirement, then we find that under the requirement that the truth-value *truth* be determinable, nine cases qualify as possibly extensional: none in IIA; five in IIB, namely, 1a, 1b, 1c, 1d, and 1e; none in IIC; and finally four in IID, namely, 1b, 1c, 1d, and 1e. On the other hand, if we relax the requirements so as to demand only that the statements have the value *falsehood* determinable by the truth-value of the components, then we have fourteen possible cases: two in IIA, namely, 2b and 2d; five in IIB, namely, 2a, 2b, 2c, 2d, and 2e; three in IIC, namely, 2a, 2b, and 2d; and finally four in IID, namely, 2b, 2c, 2d, and 2e. Thus we get

twenty-three possibilities under the requirement that a genuine statement be either verifiable or falsifiable.

The figures above give us only the number of interpretations of the original statements which satisfy each of the second set of necessary conditions for legitimately raising the question of extensionality. Whether any, and if so which, are extensional we do not thereby establish, but we do make clearer what would be established if anything were established regarding extensionality. Thus it is no simple question to ask, "Is *S* extensional?" And, obviously, it is no simpler to answer than to ask this question.

It may be interesting to attempt a specific analysis. I shall take "Socrates died because Socrates drank the hemlock." It will perhaps be admitted, as it is generally affirmed, that this statement is not extensional with respect to its components, in the sense I(1), in which component = part, for any number of replacements of the statements by others having identical truth-values will give statements having truth-values different from that of the original. Thus while it is plausible to hold that the original statement is true (but what if it be not even verifiable?), no one would hold that, for example, "Columbus discovered America because Eisenhower is a General" has the least plausibility; and this would seem to show that the original statement is not extensional. However, the moment we consider the possibility that the statement parts may be pseudo-components (I do not, of course, assert that they are) or that there may be other components than those visible, i.e. there may be virtual but non-apparent components in the sense explained earlier, the question once more becomes open.

Thus if we adopt RI(1) of "component", i.e. that S' is a component of $S \leftrightarrow S \text{ e} \rightarrow S'$, it becomes evident that there is more to the components of the causal statement than appear among the parts, even if they too are genuine components. The most obvious additional component will be that which expresses the usual requirement that a cause be not later than its effect. If we add to the list of necessary conditions for the truth of the causal statement under discussion the particular statement expressing this requirement for this case, namely: "Socrates did not drink the hemlock after he died", we see that we were somewhat premature in coming to the decision we did on the grounds given, for we did not in sense RI(1) have a complete list of components of the statement, and lacking this our analysis would have been faulty.

Our analysis is, of course, still faulty even with the addition of the third component. (It is evident that "Socrates died" and "Socrates drank the hemlock" are, in sense RI(1), both genuine components, for it surely could not have been the case that he died because he drank the hemlock if he did not die, nor could he have died because he drank the hemlock if he did not drink the hemlock at all. Thus both these statements are genuine components of the statement in sense RI(1), since they express necessary conditions for its truth.) The analysis is still faulty because more is required than the truth of these three statements for the truth of the original statement. We (should?) normally assume that for the truth of a causal statement it is required that there be some kind of law connecting the alleged cause with the alleged effect. Thus unless it were thought to be true that whoever drinks a certain quantity of hemlock (under certain conditions and of a certain strength) dies, or unless it were thought that most people soon thereafter die who undergo this experience, we should probably refrain from affirming with any confidence the causal judgment here under discussion.

Without pretending to anything like a final decision regarding the adequacy of our requirements, let us assume that we should require as a necessary condition for the truth of our causal statement the truth of the nomological statement: " (x) [x drinks hemlock (of a certain strength under certain conditions) $\rightarrow x$ dies (within a certain time in a certain manner)]," and that it be understood (a) that we exclude that interpretation of such a sentence which would permit it to be satisfied vacuously, i.e. provided no one drank hemlock or everyone dies, and (b) that it be interpreted in such a manner that we should consider it highly confirmed (quasi-verified) if a large number of men had died upon drinking hemlock and that none had survived this experience. Under such conditions we might well *consider* that the causal statement would be true if and only if these four conditions were satisfied.

We should under these assumptions have given an analysis of the meaning of the statement which shows it to be synonymous with the above four-member conjunction. But this would be equivalent to having shown the causal statement to be extensional (truth functional) with respect to its components, in the sense (RII) of "component", even though this demonstration would leave quite unaffected the view that such statements are not extensional in the sense of component = statement part, which no doubt is what is intended when,

as is normally the case, it is denied that causal statements are extensional.

Further distinctions, some possibly of interest, might be made if we were to raise here the question touched upon earlier, viz. whether univalent statements should be subsumable under the extensionality thesis. That is, we might wish to require of extensional statements that they be fully analysable, have both necessary and sufficient conditions of truth specifiable. In this event, on the basis of the above analysis we should still not have demonstrated the extensionality of our causal statement, nor would we have demonstrated the opposite. We should have shown merely its inapplicability, in the sense indicated earlier. For, as we saw, we cannot specify ascertainable sufficient conditions for the truth of the causal statement, since, as we assume, the nomological statement to the effect that all who drink hemlock die, which is a component of the statement, itself lacks sufficient truth-conditions, according to the usual interpretation of law statements as non-finite.

We might, however, decide to treat as *practically* fully analyzable statements which fail of full analyzability (assignability of the necessary and sufficient condition for their truth) only by virtue of containing at least one statement component which falls short of full analyzability in that tenuous sense in which nomological statements fall short in this respect, i.e. only by virtue of being indefinitely confirmable, although not verifiable. But we need not here concern ourselves further with these more refined issues.

Enough has perhaps been said above to indicate the drift of my views on components, etc. Whether they are of any interest, or sufficiently free of blunders to merit serious consideration, granted the interest, I confess myself unable to say. I shall be pleased if anyone finds what I have said worthy of consideration or refutation.

NOTES

¹ In all that follows, the term "meaningful" and its variants are to be understood in the sense of "cognitively meaningful".

² I take for granted here the relativity of *meaning* and *meaningfulness* to persons and contexts, so that we should strictly speaking have to say for example: (1) *S* means *X* for *P* in *C*, (2) *S* is meaningful for *P* in *C*.

- ³ (a) I write in italics the letters representing the names of sentences, reserving as far as possible the use of quotation marks for forming the names of expressions actually written out.
- (b) I ignore here the point that " S is true $\leftrightarrow S$ " holds only provided there exists a language containing the sentence S .
- ⁴ In the attempts at definitions that follow I adopt the formal mode with considerable reluctance, for while it is necessary in order to make one's formulations precise and one's sentences controllable, nevertheless almost invariably one arrives at unwanted counter-intuitive consequences that then have to be eliminated, if possible, by inelegant *ad hoc* provisions; these inelegancies themselves of course counting in the eyes of many as shortcomings in the definitions. It seems that our intuitions are normally of a rather shallow depth of intention, and that when we seek precision we find ourselves committed beyond our depth. I do not know how to avoid this dilemma, nor, it seems, does anyone else.
- ⁵ This definition is one of the grounds for not being wholly satisfied with these formulations, for it seems according to it, together with the above explanation of " $e \rightarrow$ " that, say (a) "T is equiangular" and (b) "T is equilateral" would be synonymous statements. But this certainly is inconsistent with what is here intended. The sense of "truth-conditions" involved does have as a consequence, however, that (c) "T has sides of equal length" is synonymous with (b). Thus mutual entailment, as " $e \rightarrow$ " is explained above, is only a necessary but not a sufficient condition for synonymy of statements. Hence if definition 7 is to be retained it will be necessary to understand by " $e \rightarrow$ " a relation stronger than that indicated in the above explanation, which, it will be observed, was not given as a full definition in any case. While in the above formulations I have attempted to explain the concept of truth-conditions in terms of the standard logical language, I do not, as I said earlier, believe that this is really possible. I believe rather that in the final analysis it is necessary to base logic on semantics, and not vice versa. I believe the concepts of truth and of truth-conditions to be more basic than, and to be presupposed by, those of logic. Hence in definition 7 I should hold that ultimately " $e \rightarrow$ " is to be defined in terms of "stc" and "ntc", and not contrariwise. This will no doubt be unsatisfactory to many able thinkers for whom syntactics is more basic than semantics; but there is perhaps no help for that. In the case of the above sample statements (a) and (b) it might of course be argued with some merit that they do not as a matter of fact in and of themselves mutually entail one another, but only in conjunction with the other premises constituting the system of geometry from which their logical equivalence is deducible; but I shall not make an attempt to show that in general this is a valid way of avoiding the difficulty here under consideration.
- ⁶ For some qualifications see my "Vindication of $L^*g^*c^*l P^*s^*t^*v^*sm$ ", *Proceedings and Addresses of the American Philosophical Association*, Vol. XXX, 1957, p. 60ff.

DISCUSSION

WHAT DOES MR. TENNESSEN MEAN, AND WHAT SHOULD I SAY?

Referring to Professor Tennesen's article "What Should We Say?" (*Inquiry*, vol. 2 (1959), pp. 265—90), Mr. Stigen argues that Tennesen fails to distinguish between the speech situation of the speaker and that of the interpreter. He therefore, according to Stigen, confuses the problems relevant to each of them and frequently treats problems of "What should I say?" with considerations relevant only to interpreters, whose proper question is "What does he mean?", and *vice versa*. Among other mistakes, according to Stigen, this failure to distinguish the problems leads Tennesen to recommend a Humpty Dumpty attitude for speakers, although it is appropriate only to interpreters. The paper closes with some remarks on the use of modifying expressions.

In his paper "What Should We Say?" (*Inquiry*, vol. 2 (1959), pp. 265—90), Professor Tennesen purports to "furnish arguments for a Humpty-Dumpty attitude to language" (277). Does he himself adopt such an attitude in his paper? No. Why not? Because, as Lewis Carroll's account of the exchange of words between Alice and Humpty-Dumpty convincingly shows, the Humpty-Dumpty attitude leads to a breakdown of communication at every point. In Carroll's story only one of the parties, Humpty Dumpty, adopted this attitude. What would happen if both parties adopted the Humpty-Dumpty attitude?

Tennesen tries to show that pre-school children do in fact have a Humpty-Dumpty attitude towards language, that they feel no restrictions as to what a given word may mean. He tries to show this by pointing to the fact that children are able and willing to call a dog a "cow" — at least for the purposes of a game, although Tennesen does not make this important reservation. But does this experiment prove, as Tennesen will have us believe, any "general skepticism towards language" or any "marked sensitivity to language ambiguities", two of the characteristics of the Humpty-Dumpty attitude? Not in

the least; once the children had grasped the rule that "cow" should now mean dog, the words were considered just as unambiguous, as rigid, in meaning as if the meaning of "cow" had never been changed at all. The majority of the children did not play the game on Tennesen's professed principle that "any transmitter may transmit any message" or that "nothing controls what [one] *can* (is able to) say and mean" (266).

Moreover, Tennesen's own reaction to those children who did show signs of following his principle reveals convincingly the impossibility of the use of such a principle in communication. Those children who did in fact use "cow" ambiguously, i.e. for both dog and cow, or sometimes for dog and sometimes for cow, are reproached by Tennesen as "absentminded" and "easily distracted young scatterbrains" (270), as those who "lost the thread". Which thread did they lose? The rule of the game, laid down by Tennesen, viz. that "cow" should now have a fixed, determinate, rigid meaning, i.e. dog. No game *can* be played on Tennesen's *professed* principle, no game can be played without rules. The majority of children seem to act linguistically on the principle that *a given word must mean one definite thing and not any thing*. But what this "definite thing" is, may be changed in a number of ways, say, by Tennesen giving a new rule about how to use the word, *at least* for the purposes of a game.¹

Tennesen's version of the Humpty-Dumpty attitude is in fact not appropriate to language *senders*, only to language *receivers*. The three characteristics of the attitude (enumerated on p. 265) clearly show that it is an attitude proper to people who listen, interpret and receive words, not to speakers, communicators and senders. But Tennesen fails to draw the speaker-listener distinction and thereby confuses both himself and his readers. Throughout his paper he therefore discusses the questions about what one can "ever *say and mean*" (277, cf. 266) as if they were similar. The problems of *saying* and those of *meaning*, at least when they arise in contexts of linguistic communication, are basically different. Problems about *saying* primarily arise when there is no problem about what is meant,² and problems of *meaning* arise only when there is no problem about what has been said. As long as I don't mean anything (not even that I have no opinion on an issue), I had better keep my mouth shut, and as long as I am not clear about what his words were, I had better abstain from speculating about what his words meant. The problem of what *we* should *say and mean* should, therefore, be divided up into two distinct problems: The problem

of what *I* should *say* (when I mean something), and the problem of what *he* (she, they) *means* (when he says something). The problem of the speaker is the problem of "What should *I* say?", and I never wonder about what *he* should say; that is his problem, as speaker.³ The problem of the interpreter or listener is the problem of "What does *he* (she, they) mean?", and I never as listener wonder about what *I* mean.⁴

If this is so, it becomes clear that e.g. "language questions", problems about the correctness of *sentences* from a "grammatical-syntactical or idiomatic point of view" (280) — i.e. T₂₃ and T₂₄ in Tennesen's list of interpretations of 'Should we ever say . . .' (279) — are problems of speakers only, never of listeners, whereas e.g. "tenability questions", problems about the correctness of *statements*⁵ — i.e. T₂₅ in the list (279) — are problems of interpreters only, not of speakers.⁶

Whether or not this is correct, it would seem that any questionnaire which does not make it clear to the respondents in which role they should imagine themselves — speakers or interpreters, senders or receivers — would foster pseudo-agreement and -disagreement. A question like "Should we ever say x?" is tricky because it does not present us with a genuine speaker situation where a speaker has a message or intention to convey, and where the problem is just how to say it. This defect is probably what J. L. Austin tries to avoid when instead he asks "What should we say when . . .". That is, he describes as carefully as he thinks needed the situation in which the speaker finds himself — without suggesting the very words under investigation.

Thus, on the voluntary-involuntary distinction, to present the respondents with a story making it clear to them that what we have is a *standard* case of yawning, and then to ask them the question "Did he yawn voluntarily (or clumsily)?", would be to change the ordinary case to a special case, viz. to suggest that there is a reason to ask that question, thus throwing suspicion on the normality of the case. Even if the suspicion is rejected, a new aspect has been introduced by the modifying adverb — an aspect not adhering to "yawning" simply. Tennesen maintains that in teaching children or foreigners the meaning of "to yawn voluntarily" we would point to a standard case of yawning. And indeed I might *point to* a standard case, but I would certainly not *describe* it in the ordinary way, i.e. *as* a standard case (which would be a description of the situation which I express by "he yawned", not by "he yawned voluntarily"). For if I have first

taught my pupil what “yawning” means by describing a standard case of yawning, my pupil would certainly object to my explaining “*voluntary* yawning” or even “*ordinary* yawning” by exactly the same description (of the same case), or else I would have to say that “voluntary” and “ordinary” do not add anything to “yawning”. And in a way they do not, for the same yawn may be both just “a yawn”, “an ordinary yawn” and “a voluntary yawn”. What “voluntary” and “ordinary” add is nothing to the action, but something to my way of looking at it and describing it, another aspect. To modify an action there need not be any *real* aberration from the normal case; it is enough that I consider the case with that question in mind. Words like “ordinary”, “normal”, “voluntary” cannot be learnt by standard cases alone; what is ordinary, etc., can only be determined and understood as such against what is *extraordinary*, etc.

The case of nouns, which Tennessen raises, is different, since nouns do not usually modify, like adverbs, and some adjectives. Still, nouns might well be regarded as modifiers, in the way Spinoza regarded the finite extended things as modifications of one of the attributes of God, extension. Instead of “chairs” we should then rather talk of “chairy extension”, and the above principle would hold equally true in this case: As long as there were nothing peculiar to note about extension — that is, as long as we were faced with a standard case of extension, or just “space”⁷ — there would be no reason to talk about “chairs” or any other finite things. To teach a student of Spinoza what *unmodified* extension is, we should have to explain to him that it is extension with no finite things in it. That is, to talk about unmodified extension is to suggest the possibility of finite things, like chairs, or the aspect of modes, although *in reality* unmodified extension is nothing but just plain extension. In the same way, to explain what a voluntary action is, is to suggest the possibility of involuntariness, if only in order to reject it, just as in teaching someone what an ordinary action is, I have somehow to suggest the possibility of extraordinariness, although in reality an ordinary action is just a plain action, and nothing more.

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¹ Occasionally, Tennessen presents a much more plausible thesis, viz. that when I have a message to convey, I may *freely* choose among a number of more or less *appropriate* expressions (266). It is to be regretted that he does not go on to ask the primary questions that naturally arise out of this thesis: How do I decide which

- expression (word, sentence) is the most *appropriate*? In what sense have I a *free* choice here?
- ² Of course, there are apparent exceptions. I say "I don't know what to say" sometimes to express that I do not know what I mean.
- ³ Apparent exception: I sometimes do say "What can *he* say?" But then I put myself in his place, considering his problems as a speaker.
- ⁴ Apparent exception: I sometimes do say "What did I mean?" But then I imagine myself as the receiver and interpreter of my own words.
- ⁵ That Tennesen does not distinguish between *sentences* and *statements* is a consequence of not separating the problems of the speaker from those of the interpreter. For what the speaker has to consider in order to determine what he should say, are *sentences*, whereas what the listener has to consider in order to determine what is meant, are *statements*.
- ⁶ Again, to forestall a likely objection: I may ask myself "If I say x, will it be true?" before I have said something. But in that case I ask that question not *qua* speaker but as I turn over in my mind various interpretations of a sentence which has suggested itself to me.
- ⁷ We cannot say "*empty* space" because that suggests the possibility of filling the space with finite extended things.

VINDICATION OF THE HUMPTY DUMPTY ATTITUDE TOWARDS LANGUAGE

A rejoinder to Anfinn Stigen and other critics

Effective objective (*sachlich*) verbal communication is dependent upon the use of linguistic locutions which are: a) suitable for some special purposes, b) clear (*i.e.*, having a satisfactorily high degree of subsumability), and c) in accordance with some ordinary (*i.e.*, frequently occurring) language usages. Only in so far as point c is concerned is a study of actual language usage of (indirect) value to philosophers. And this holds true regardless of whether one's underlying assumption tends towards the view: 1) that ordinary language is perfect (Oxford), or: 2) that ordinary language is a mess (Oslo). In any case, one needs to know about the most ordinary usages to prevent unnecessarily drastic deviations from them. Drastic deviations may mislead the sender, as well as the receiver, create communicational disturbances, misunderstandings, and confusion (*vide*: Strawson's use of "presupposition"). However, considerations of a) suitability for special purposes, and b) clarity (subsumability) will most often, if not always, prevent a communicator from flatly adopting any one of the existing language usages of a given important linguistic locution. He would feel the need for: "explications," "rational reconstructions" or conceptual alterations of one kind or another. In fact, there are instances where the sender finds it most advantageous to disregard completely ordinary language (*vide*: Einstein's use of "simultaneity"): He "makes words mean what he wants them to mean." This is the Humpty Dumpty *sender* attitude towards language. The corresponding *receiver* attitude manifests itself as awareness of and tolerance for language ambiguities.

Humpty Dumpty had just explained to Alice the advantages of receiving *un-birthday* presents rather than just birthday presents:

"... there are three hundred and sixty-four days when you might get un-birthday presents".

'Certainly', said Alice.

"And only *one* for birthday presents, you know. There's glory for you!"

"I don't know what you mean by 'glory,'" Alice said.

Humpty Dumpty smiled contemptuously. "Of course you don't — till I tell you. I meant 'there's a nice knockdown argument for you.'"

"But 'glory' doesn't mean 'a nice knockdown argument,'" Alice objected.

"When *I* use a word", Humpty Dumpty said in rather a scornful tone, "it means just what I choose it to mean — neither more nor less."

"The question is," said Alice, "whether you *can* make words mean so many different things."

"The question is," said Humpty Dumpty, "which is to be master — that's all."

When Russell maintains¹ *that we do not have any reason to assume* that the sun will rise tomorrow (or that a man who jumps out of the Empire State Building will fall down and not be drawn skywards), we all feel tempted to chime in with Paul Edwards²: "What on earth do you mean by 'reason' (or 'assume')?" Much the same, I believe, holds true of any first and off-hand encounter with *e.g.*, Einstein's unprecedented concept of 'simultaneity' or, say, the rather eccentric usage of "presupposition" in some of the works of Strawson.³

We would in all cases be inclined to remark à la Alice: "I don't know what you mean by: 'glory', by: 'evidence', by: 'simultaneity', by: 'presupposition'." . . . And Humpty Dumpty and Russell and Einstein and Strawson would smile: "Of course you don't — till we tell you." They all have, it seems, the same attitude towards language: whenever they use a word, they aim to make it mean just what they choose it to mean — neither more nor less: *the Humpty Dumpty approach* to the problem of how to determine *what we should ever* (or ordinarily) *say when, and what we should never* (or not ordinarily) *say when* . . . This free and manipulating linguistic attitude is important to secure effective communication between *all* communicators, regardless of whether their role in a communicational event may be most adequately described as being predominantly that of a *sender* of a certain transmitter, or that of a *receiver* of this transmitter. In the latter case the Humpty Dumpty attitude will manifest itself as an extreme degree of hermeneutical tolerance and imagination due to the complete lack of linguistic super-ego; whereas *the sender-side* of the same attitude is chiefly characterized by the communicator's clear awareness of his freedom *in principle* in choosing words and their meanings. However, the mere feeling of complete freedom of choice, of course, by no means prevents a communicator from making an inappropriate, inadvisable choice. When, for instance, a communicator introduces a word and gives it

a weird, absurd sounding meaning, uses it in a sense in which it has obviously never been used before (cf. *e.g.*, Russell and Einstein above), *but without anything useful being gained*, as in the cases of Humpty Dumpty and P. F. Strawson, then I should certainly feel free to expostulate with any such communicator about his choice of words and meanings and try to persuade him to make a better choice. But, I shall never say: "You *cannot* say (utter, or mean) such a thing," unless the context makes it clear that what I want to transmit by "you *cannot* . . . etc." could, somewhat less misleadingly, have been expressed by: "You are a fool: saying such a thing!" or more specifically: "By this usage, you only add to the potentialities of misunderstandings without suggesting or contributing anything to a more explicit or consistent language usage." In general one might say that a word or an expression is introduced in an advisable, commendable, appropriate . . . manner if it covers, fairly unambiguously, a fruitful concept, suitable to some end in view, without departing too radically from the most common, ordinary usages of the word or expression in question. Needless to say, the communicational appropriateness is by far the most important consideration. On the other hand, if one intends to ignore completely common language usages, one should, I feel, in most cases rather think of inventing a new linguistic expression to cover the new conceptual notion.

Quite illustrative is the Russell/Strawson controversy. Russell's Theory of Descriptions, as everybody knows, suggests that one should conceive of such sentences as: "The present king of France is wise," as a conjunction of two assertions (of which at least one is false). It has always been clear⁴ that whatever advantages this proposal might have, they have nothing to do with analyses or hypotheses about (common or "ordinary") language usages. None the less, the following passage is found in Strawson's "On Referring" (p. 330):

Now suppose some one were in fact to say to you with perfectly serious air: 'The king of France is wise.' Would you say, 'That's untrue'? I think it's quite certain that you wouldn't.

Strawson is wrong: Of about 1,500 informants tested in some recent experiments no one seemed to act in accordance with Strawson's predictions.⁵ But, what is more: even if Strawson's view had been sound and supported by empirical evidence, his objection would still be completely beside the point. Even Strawson, I assume, would agree

that if I say: "Peter and Jack are out in the hall," and I open the door but find only Peter (not Jack) there, I should be willing to admit that the whole statement about Peter *and* Jack is false. However, my willingness to admit that I was wrong would show an inverse correlation with the number of persons included in my statement. If my hypothesis was concerned not with two, Peter and Jack, but with twenty or two hundred or two thousand persons, the names of whom I would rattle off and claim that their bearers were out in the hallway, then I should most certainly deny that I was wrong if it was found that one (or two? or three? . . .) of the 2,000 persons was missing. One should have to employ a rather forced and artificial language in order to say: "Your statement was false: one of the 2,000 you mentioned was not there." On the other hand: such linguistic preferences and habits would by no means prevent us from imaging circumstances under which we would gladly adopt this forced and artificial language for the sake of conceptual clarity, consistency — or for other good reasons.

Strawson, one might say, has opened the door a crack to the vast field of empirical investigations of language, taken a peep in, and, after (almost immediately) having shut the door, he reports: "Russell is wrong: The Theory of Descriptions is fatally *incorrect* because one would not (could not? should not? ought not to?) utter, and/or mean: "The present King of France is wise is false'!" However, in his own attempt at a "solution to this puzzle" Strawson seems absolutely uninterested in what "one" would or would not say, and introduces a notion of 'presupposition' with probably no footing whatsoever in any existing language usages.⁶ One might feel tempted to ejaculate impatiently: "What is it going to be: The Humpty Dumpty attitude towards language or one of the other approaches?"

Which are "the other approaches"?

2. The Ineffability Approach

In "What Should We Say" I briefly touched upon an approach to the problem of "whereof one should speak" and "whereof one should remain silent," based upon the rather murky and mystical notion of ineffability: certain "things" cannot be said or it is impossible to say certain sentences or expressions. One *cannot*, or *it is impossible* to utter, e.g.: "My necktie has a cause," "she unintentionally tied a string across the top of the staircase," "he yawned (in-)voluntarily (heartily, disgustingly, sleepily, normally, ordinarily, in a standard, paradigm-

matic manner . . . etc.),” “I’ve been recognizing you for at least three minutes, and I think I am about half done,” “I promised my chair to stop smoking” . . . etc. What most plausibly may be meant by: “cannot”, it is impossible” in such and similar contexts is quite problematical if not completely obscure. However, it has been claimed that there may be some thin connections between, this linguistic ineffability approach to the (dis-) solution of presumably philosophical problems, and what may be labeled “linguistic rigidity”. “Linguistic rigidity” is used to designate certain attitudes to the use of language alleged to be predominant in so-called “primitive” people as well as in pre-school children. It is generally characterized by an inability to utter certain words or sentences, “to play with words,” “to look on language as a word game,” and “make words mean what we want them to mean.”

Humboldt tells a story about a peasant who, after listening to two students of astronomy talking about the stars, said to them: “I can see that with the help of instruments men could measure the distance from the earth to the remotest stars and find out their position and motion. But I should like to know: how did you find out the *names* of the stars?” He assumed that the names of the stars could be found out only from the stars themselves. Vigotsky⁷ claims that similar dispositions may be found in every one of us, if we go back to the pre-school age. The experiments with pre-school children, reported in my article, show, however, that neither children nor grown-ups have inhibitions in playing with words and making words mean what we choose them to mean, *e.g.*, in saying about a dog: “it is a cow,” or about a basketball: “it is a bicycle”, — and so forth, as soon as it is made clear to the informants that by “can you call (say)?” is meant: “are you capable of making the following sounds?” (*i.e.*, “uttering the following sentences.”) In the pretest to the above mentioned experiments this was easily accomplished by the following procedure: When the (pre-school) child entered the room, the investigator said: “Can you call a basketball ‘a bicycle’?” Child (*e.g.*): “No!” Investigator: “Watch me: *I can*,” and pointing in the direction of a basketball he uttered: “Bicycle!” Then, turning towards the child again: “See how easy it is to call a basketball ‘a bicycle’? Now you try!”⁸ Of course, one found absolutely no trace of any so called linguistic rigidity in the pre-school children or in any of the tested age groups of normal informants. What one found was a rather obvious ambiguity in such formulations as: “we can (not) say (call, convey, or utter),” “it is (im-)possible to

say (call, convey, or utter)", etc. Reactions classified by Vigotsky, Piaget, Frazer and others as symptoms of linguistic rigidity seem to be due to nothing more exciting than a tendency — among investigators as well as respondents — to oscillate in their interpretation of the mentioned formulations between the following two directions of interpretation: a) "it is (im-)permissible, (dis-)advantageous, (in-)advisable, (un-)fitting, (un-)fortunate, (un-)reasonable . . . to say (call, convey, or utter)," and: b) "it is (un-)achievable, (un-)attainable, within (resp. beyond) my powers and capacities . . . to say (call, convey, or utter)." The former direction of precization (a) seemed in most cases to be preferred to the latter (b).

3. The Impermissibility Approach

A far more prevalent and prominent approach to the question of "what should we say when" than the ineffability approach, but not always separable from this, may be called "the impermissibility approach."⁹ Certain locutions are flatly rejected, forbidden. They are: "sins against the word," "sins against language," "nonsensical," "contradictions," "logical oddities," "(self-)contradictions," "there is a rule against saying them," "Sie waren in der Sprache nicht vorgesehen," etc., etc. Various devices have been proposed for separating allegedly impermissible from permissible and legitimate locutions — of which two will be dealt with here:

3. 1. The Method of Revelation

The easiest way by far, is simply to "see" or "hear" or otherwise *perceive* the permissibility or the impermissibility of a given locution. A native speaker, it has been maintained,¹⁰ will never (or rarely) be in doubt. He *perceives* the (im-)permissibility directly, instantaneously, in a flash of revelation, by some sort of linguistic instinct, logical sense, hermeneutical clairvoyance

Oddly enough these seem to be qualities most often found in precocious children, and pedantic logicians:

The girl next door to where we live had a fight with her brother the other day and complained: "Ronny is so rough, tomorrow I'll be black and blue all over!" To which my little boy dryly remarked: "No single object can at the same time be black and blue all over." Needless to say, he also vigorously objects to such sayings as: "Enough is enough,"

"I both like it, and I don't," etc. Philosophers do the same. Thus, Nowell-Smith¹¹ labels as "logically odd" and impermissible the sentence: "P is cultivating weeds" which, of course, is the most customary, ordinary, legitimate way in which to describe a very natural and important horticultural activity undertaken by Standard Oil and other anti-weed-spray producing companies. Likewise, it is the same possibility of misinterpretation which makes the well known advertisement from a cleaning establishment worthy of being reprinted in a comic paper: "Don't kill your wife; let us do the dirty work." All the cited cases have one thing in common: every communicator involved understands perfectly well how the sentences concerned are most plausibly to be interpreted. The impermissibility direction of interpretation is rendered possible only because of deliberate or grossly negligent misinterpretations. In the philosopher's case cited, and in similar examples of other philosophers, it seems as if the impermissibility direction of precisization prevails due to a widespread optimism among philosophers that sentences or locutions may profitably be judged by their appearance, so to speak, that the external form should *reveal* or otherwise permit one to "*see*" or "*hear*" whether a given locution is idiomatic, in accordance with "the syntax of language," "the rules (or laws) of language," in short, whether it is: *permissible* or: *impermissible* (e.g., a contradiction, a tautology, a logical oddity . . . etc.)

What I *inter alia* tried to show by some of the more elaborate empirio-semantic experiments reported in "What Should We Say?" was that a short lecture (207 words) will suffice to awake this presumably dormant hermeneutical clairvoyance and turn perfectly normal human beings into rigid, *logical pedants*. On the other hand, a slightly longer lecture (255 words) was apparently enough to induce a more commonsensical *latitudinarian hermeneutical attitude*. When confronted with sets of (25, 32, and 48) sentences, the *logical pedants* intuited, without difficulties or exceptions, the classification of all the given sentences in either one of the following three categories of impermissible sentences: 1) (self-)contradiction, 2) tautologies, 3) nonsense sentences, whereas the *latitudinarians*, on the other hand, were equally capable of (or insisted on) interpreting all the same sentences to transmit plausible, reasonable, fairly interesting, worthwhile, more or less tenable hypotheses, or more or less advisable proposals.¹²

Quite illustrative in this respect are the customary cases where allegedly impermissible linguistic locutions are scouted out by some "logical sense" or linguistic instinct, etc. Take again the hackneyed

example, T_0 "It is raining outside and I don't believe it." To our latitudinarians, of course, the cited sentence is just as permissible (and easily understood) as, T_1 "It is raining outside. How extraordinary!" And what is more, it seems exceedingly easy to imagine T_0 occurring in (rather trivial) contexts where the most rigid logical pedant would have difficulties in laying his finger upon its impermissibility. A few hours ago, shortly after I started writing the present paper, it was, contrary to all reasonable expectations (rain in June in California!) actually raining outside. Absorbed in my work I did not notice, and had I been asked, "Do you believe it is raining outside?" I would honestly and sincerely have answered: "No," that I did not believe it was raining outside. Thus, if a few hours ago I had said to myself: "It is raining outside while I don't believe that it is raining outside," I would have made a true assertion. How I could possibly have gained such an insight into the discrepancies between my beliefs and the actual state of things, is another problem. But one thing I know. Whenever I have any such extraordinary insights and feel the necessity for conveying them to myself or others, I shall feel free to make use of any locutions — how even weird or bizarre they may look to a pedantic logician — if they only provide an objective and expedient verbal transmission. In light of these reflections it is not altogether obvious why anybody ever could find anything odd about such *fairly* clear and, in most contexts, readily understood sentences as: "It is raining and (or: but) I don't believe it," "It is Thursday and (but) I don't believe it," etc. . . . To classify such sentences as "nonsensical," as: "impermissible locutions," "logical oddities," "contradictions,"¹³ ("tautologies," respectively), "sins against language," and what not, is really a sin against language. It prevents language from fulfilling its major purpose (*i.e.*, to increase inter- and intra-personal communicability) by preventing adequate locutions from being employed when effective communication is entirely dependent upon their availability. This is why it seems to me safe to assume that in most cases so-called "contradictions," "logical oddities," etc. are in fact nothing but, for various reasons, infrequently occurring locutions! Their essence is simply to be infrequent.

3. 2. The Infrequency Approach

When John Austin was in Norway, October, 1959, he is reported to have made the following statements¹⁴:

That there are rules prohibiting something from being said, does not say more than that we never say so and so.

We say there is a rule against saying X, when X is never said.

It is rather unlikely that Austin in the above quotations merely meant to make explicit his own, and maybe a few other Oxonian philosophers', peculiar use of the word "rule." Another and in all likelihood more plausible interpretation may be expressed as follows: "Things which are never said, ought never to be said; they are impermissible locutions, 'sins against the language'," etc. In any case and however the cited sentences are interpreted, it seems safe to assume that there are detectable differences between the indicated, Austinian point of view, and mine. I claim that: "The mere fact that a certain way of speaking has worked satisfactorily for a number of millennia creates but a feeble presumption of present and future desirability *i.e.*, expediency for purposes of communicability. By the same token, *the non-occurrence or extraordinarily infrequent occurrence of a certain locution offers per se no clue or argument for its impermissibility*," as is aptly illustrated by the following anecdote:

A group of Norwegian farmers were celebrating Christmas. The Aquavit bottles stood lined up on the table in front of them. All were drinking heavily and silently. The quiet of the night was broken only by the jingling noise whenever another empty bottle was thrown out the window. The turn had just come for the tenth bottle to whistle away when one of them, who for ninety years had been known to be a deaf-mute, suddenly remarked: "There is still something left in the bottle." The others turned to him with wondering glances: "But we thought you were deaf and dumb?" "Not quite," he replied laconically, "I just haven't hitherto found anything worthwhile to say." This story isn't authentic. Even among silent Norsemen an attitude like the one described above would be found quite unconventional, one might even say eccentric. However, it clearly illustrates my point: to the general question of "What should we say when?," there is but one satisfactory general answer: "That which is worthwhile saying."

What, then, is worthwhile saying when?

This question, of course, does not readily lend itself to a flat and general answer. It seems, however, off-hand that we may normally or generally exclude as insignificant any sentence which, within an intended communicational frame of reference, is plausibly and invariably interpreted to convey propositions commonly conceived *either* obviously true (or advisable) *or* obviously false (or inadvisable) within this frame

of reference,¹⁵ the major exception being sentences transmitting propositions which are not by themselves contentious (within the intended frame of communication), but employed in order to support or weaken genuinely contentious propositions. With this, and maybe a few other exceptions, there can hardly be any doubt that: *we generally do tend to avoid saying the obvious, — be it an obviously true or advisable, or an obviously false or inadvisable proposition*: Neither is worthwhile saying!

In terms of the following diagram my thesis may be rephrased in this manner: As a rule we try to utter or write *significant* sentences; *i.e.*, sentences expressing propositions the tenability (advisability, respectively) of which may be: a) (strongly) supported by new evidence (and/or reasoning) brought forth by the sender of the sentences, *and* b) questioned by at least one person in at least one situation within the sender's language society or intended frame of communication. Therefore: whenever a sentence, T, in a normal, standard case of communication, is interpreted to convey a non- (or anti-)significant proposition, we may ordinarily rest assured that we have to do with a forced and far-fetched, out-of-the-way, implausible interpretation T.¹⁶ From this follow chiefly two things: a) there are innumerable reasons why apparently infrequent locutions are "never said," b) *if* such locutions are employed, they are invariably well understood in all ordinary, common cases of communication.

Sentences conveying:		
	AUDACIOUS propositions	TRIVIAL propositions
Sentences conveying:	+ + Significant sentences	+ - Non-signifi- cant sentences
	- + Non-significant sentences	- - Anti-significant sentences

I shall in conclusion say a few words concerning the first part (a) and deal particularly with certain disadvantages of regarding infrequent linguistic locutions as impermissible.

We normally tend to avoid propounding obviously untenable hypotheses or inadvisable proposals. We should never say: "the interior

of Sirius is filled with mock-turtle," because Sirius is never filled with mock-turtle. However: if there were linguistic rules prohibiting all sentences which at time, t , were unanimously interpreted to express untenable hypotheses (or inadvisable proposals, respectively), the same rules would prohibit (or, at least, inhibit) effective communication at a time, t' , when the world had changed in such a way that certain "plain" (but interesting) matters of fact would be most adequately described (only) by employing some of the very (at time, t ,) interdicted sentences. Take for example the sentence: "I had lunch in Oslo at noon and breakfast the same day (two hours earlier) in San Francisco," and let t be 1869 and t' 1969. And add to this the fact that in many such cases, the change of the world would entail a change of language. Not only is this true for technical inventions (Imagine Samuel Pepys describing the interior of a modern kitchen!) More important are the cases when locutions, commonly considered "contradictory" at t become commonplace at t' ; consider *e.g.* the expression: "to split an atom"! It would, by the same token, be silly of us to ban forever such an expression as: "a rate of speed exceeding the velocity of light," or the like.

What is said above about sentences conveying obviously untenable hypotheses (and inadvisable proposals) will, of course, apply to such sentences as well which are unanimously interpreted to express commonplace platitudes or other uncalled-for propositions. Thus if, in a perfectly normal situation a normal person is yawning a normal yawn, it is true that we should never say: "That person is *not* yawning now," *nor* should we ever say: "That person is yawning." If such sentences are uttered (under such circumstances), the audience would wonder: Why on earth does he want to say this? What is he up to? What will his next move be? He must have something up his sleeve. What is it that he wants to prove? In other words, the sentences have ceased to have symbol function. Because: Nobody wants to say something everybody can see is false, and: Nobody wants to say something everybody can see is true. Consequently: "He is (not) yawning" is never, or not ordinarily, said unless there is something fishy or otherwise remarkable about the yawn or the circumstances under which it takes place. Needless to say, the same goes for any adjectives or adverbs we would consider using: They are all employed if only if and we have good reasons (or plausible pretexts) for doing so.

In addition to this general rule — that we only use locutions instrumental for conveying what we have found worthwhile mentioning — come all sorts of more irrational terminological preferences, stylistic

taste, aversions against pompous, flowery or technical language, etc. It is perfectly clear that if it is bedtime, I am alone, I yawn, I would never dream of registering my yawn under the heading: "an action," which may be: "voluntary," "involuntary," "deliberate," "intentional." "causally determined" or anything as ludicrously pompous as that. I might even be tempted to use a strong terminology and say: "such adjectives would be uncalled for," "they don't make sense here," or the like. Perhaps this, under the given circumstances, would go for *any* adjectives or adverbs one could possibly imagine. But it would most certainly be a mistake if it would prevent me from employing any of these adjectives or adverbs *in case* I should ever need them for an adequate description of my yawn when alone at bedtime. What is more, one could easily think of adverbs such as "normally," "ordinarily," "usually," "habitually," "consuetudinary," "paradigmatically," etc. which, when used in connection with verbs like "yawn," "kick," "croquet," "sign," etc., would form rather paradoxical sentences. *Such sentences would*, according to John Austin and others, *be impermissible, logically odd, "sins against language," nonsensical, etc. if and only if they were expressing true statements!* E.g.: "He yawned normally," "he signed the check in an ordinary (customary, standard) way" . . .

In short: There has hitherto never been a need for saying, and most likely nobody has so far said: "I met a man today who was 360 feet tall." But I know that whenever I meet such a giant, I shall *not* run home to my children and say: "Today I met a man who was . . . Oh, no, I am sorry, but I shan't be able to tell you how tall he was. Unfortunately, our language has not foreseen such eventualities." On the contrary — we need a language with which we, if necessary, could describe unforeseen, unimagined, hardly imaginable, even today unimaginable phenomena. On the other hand the language must permit one to express the most boring, futile, idle, commonplace, platitudes whenever that — for some other possibly strange reasons — seems to be important. It is certainly true that a locution is by and large most correctly applied in cases which are the ordinary, conventional, common-sensical, usual, customary, normal, standard, paradigmatic cases of its right application. However, it would be most unfortunate if one were prevented from employing it in such cases as well where it, off-hand, sounds odd and absurd, where — as some philosophers and others would say: "*it does not make sense*".¹⁷

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- ¹ *Problems of Philosophy*, pp. 96 ff.
- ² "Russell's Doubts About Induction," *Mind*, Vol. LVIII, No. 230, April, 1949, p. 151.
- ³ E.g.: *Introduction to Logical Theory*, Lnd. 1952, pp. 174 — 92 and in *Individuals*, Lnd. 1959, pp. 189—204. Cf. also: "On Referring," *Mind*, Vol. LIX, No. 235, July, 1950.
- ⁴ To everybody, including Russell himself, cf. "Mr. Strawson on Referring," *My Philosophical Development* (Lnd. 1959) pp. 238 ff.
- ⁵ I.e., almost all reacted exactly as they did to all other puzzling and ambiguous sentences: "If what you mean is this . . , then I agree; if what you mean is . . , then I disagree." No one would maintain that: "*the truth or falsity question does not arise because there is no such a person as the present king of France.*"
- ⁶ I.e.: It is extremely unlikely that anyone has ever used "presupposition" in such a way that nothing can be a presupposition (in this sense of "presupposition") and at the same time express an untenable statement. This is, as one will know, the case with Strawson's 'presupposition'. (Cf. *Introduction to Logical Theory*, pp. 174—92.) And this is why one feels that Strawson (as well as Humpty Dumpty) would have been better off with an unprecedented designation to cover this unprecedented notion.
- ⁷ L. S. Vigotsky, "Thought and Speech," *Psychiatry*, Vol. II, No. 1, February, 1939, p. 36. The Humboldt anecdote is borrowed from the same article.
- ⁸ For the actual experiments see the preliminary report in: "What Should We Say?" pp. 268—72.
- ⁹ For further details cf., e.g.: "Logical Oddities and Locutional Scarcities" *Synthese*, Vol. XI, No. 4, Dec., 1959, and in particular the forthcoming article in *Journal of Philosophy*: "Whereof one so far has been silent: . . ."
- ¹⁰ Cf., e.g., Stanley Cavell: "Must we mean what we say?" *Inquiry*, Vol. I, No. 3, Sept., 1958.
- ¹¹ In *Ethics*, p. 72. (Cf. also: "Logical Oddities and Locutional Scarcities." *Synthese*, *Ibidem*.)
- ¹² Incidentally, among the sentences interpreted, the sentence: "The present King of France is wise" had one particularly amusing interpretation. One respondent answered: "That's right, he *is* wise — because he doesn't exist. And what can be wiser these days than not to exist?"
- ¹³ It has been maintained that nobody would ever use the term "contradiction" in any such cases. This is wrong. At a session of The Nordic Summer University, Otaniemi, Finland, 1954, the sentence: "Today is Thursday and I don't believe it" was "seen" to be a "selfcontradiction" ("själv motsägelse") by Prof. Eirik Stenius, University of Helsinki and Prof. Anders Wedberg, University of Stockholm, among others.
- ¹⁴ Vide: "John Austin and Arne Naess on Herman Tennessen's experimental warning: 'What Should We Say?'" (Typewritten manus mimeographed Berkley, Calif., 1960.)
- ¹⁵ Cf. "On Worthwhile Hypotheses," *Inquiry* II, No. 3, 1959. Vide also: *Synthese* XI, No. 4, 1959, and "Whereof one so far has been silent," *Journal of Philosophy* (forthcoming).
- ¹⁶ This, of course, does not reduce the value of the awareness of the possibility of such directions of interpretations. It may come in handy, particularly in cases when we

believe we are propounding, *e.g.*, a hypothesis which is both absolutely true and at the same time a flabbergasting, breath-taking novelty. The chances are that our hypothesis is only (absolutely) true when interpreted to be a non-significant triviality (*e.g.*, analytically true or the like), and only interesting if interpreted in a direction which makes its tenability (as hypothesis) rather dubious or even improbable. Cf. H. Tennessen: "On Worthwhile Hypotheses," *Inquiry*, Vol. II, No. 3, 1959, pp. 183—5.

- ¹⁷ Recent empirio-semantic enquiries into the ambiguities of T_0 : "It does not make sense to say $X \dots$," revealed at least six main directions of precization of T_0 . Furthermore it seemed as if X was only to be disqualified as "*eine sinnlose Aneinanderreihungen von Worten*" if T_0 is interpreted in the direction of T_1 or T_2 below; whereas T_0 , for the given variable of X , is only expressing a true statement provided T_0 is interpreted in one of the other, cognitively entirely different main direction of precization of T_0 . T_1 and T_2 may here tentatively be formulated as follows:

T_1 One is expressing a logical oddity, a logical, grammatical or syntactical inconsistency, a negative or positive analytic statement — *i.e.* (self-) contradiction or tautology — or a pleonasm, a redundancy — when uttering X .

T_2 One is uttering a random cluster of words, a haphazard conglomerate of irrelative linguistic expressions or (in principle) unverifiable and unfalsifiable statements, totally devoid of cognitive content . . . when uttering X .

Cf. the authors: "On making sense"; paper to be read before the annual meeting of the American Philosophical Association, Yale University, Conn. December 1960. Here I try to argue the point that "ordinary (or normal) language" philosophers should employ modern "soft" science methodology. In another paper: "Empirical Semantics and the Soft Sciences" (read before the *International Congress for Logic, Methodology, and Philosophy of Science*), attempts are made to show how soft science methodology may profit by cooperation with empirically oriented, research minded analytical philosophers.

SYSTEMATIC INNOVATION AND SOCIAL PHILOSOPHY

Corbett's "Europe and the Social Order" makes two claims: that industrial societies are marked by "systematic innovation"; and that, as a consequence, our inherited social doctrines are obsolete and must be replaced. The first claim is true, but the second is false: it is not the case that social doctrines are "consistent" only with certain kinds of social order: we cannot therefore say that innovating societies either preclude or compel any particular social doctrines. They do not invalidate *a priori* doctrine, though they undermine belief in it. It is therefore less important to attack such doctrine than to study innovation empirically.

Mr. J. P. Corbett's "Europe and the Social Order"¹, which only incidentally concerns Europe, makes two main claims: that the social order of industrialised and industrialising societies is marked by "systematic innovation"; and that, as a consequence of this, the social ideas we have inherited have become obsolete and must be replaced by an intellectual revolution. The first claim is true and important. The second claim the author, in my opinion, does not fully substantiate: but in making it he has identified a problem which is of great importance and is common to all branches of social study: that of the consequences of innovation for received bodies of theory.

To say that innovation in modern society has become "systematic" is to say not merely that it occurs, nor that it occurs at a rate which is rapid and accelerating, but to say that it has become *institutionalised*: the dominant institutions of our society are committed to innovation to such a degree that we can only expect that the tendency to social change will continue, and will continue to accelerate. Science and technology, the great agencies of social innovation, are no longer the preserve of gifted amateurs operating alone and on the fringe of society: they are great and powerful institutions at its centre, commanding legions of trained minds and huge economic resources, dominating the universities from which until recently they were excluded, stimulated by the demands of governments and require-

ments of the economy and of war, and strengthened by and in return strengthening an ideology favourable to constant innovation. Science, when it shook itself free of the Cartesian notion that it consisted of certain truths, freed itself also of the notion that it was the private work of one man: as it became an empirical, it became a social, activity; and as it became more socialised and institution-alised, its progress ceased to be the random result of the occasional insight of genius, and has become the constant, predictable swell that results from the pooling of findings throughout the intimate universe of science. We should cease to be surprised at innovations: we should recognise that our social order is such that we must expect them. In fact our attitudes are already conditioned by the recognition of Corbett's point: we do not wonder at the novelties of science and production, as the Victorians did: we have come to expect rockets to reach the other side of the moon. There are elements of uncertainty in Corbett's prediction. He believes that systematic innovation in itself is good: and certainly the dominant ethic in every society from China to Peru endorses his view: Gandhi's cause is as lost in Asia as is Ruskin's in Europe. But it is conceivable that we may become bored with innovation, and that if we do, some sort of stabilisation may occur. The processes of change may themselves remove some of the incentives to change — as the abolition of poverty may weaken the impulse to increase production,² or as the race in armaments may produce a stalemate which will cause it to slow down. Some changes may cancel themselves out in such a way as to re-establish continuities, as e.g. the continuity of warfare is preserved by the balancing of changes in the science of defence with changes in the science of attack. However, in the short run or even the fairly long run, modern society is so much geared to innovation that it could scarcely go into reverse if it wanted to.

Mr. Corbett's argument becomes more doubtful when he proceeds to the thesis that this new type of social order entails a revolution in our social ideas. First he contends for the inadequacy of received doctrines: the doctrine of natural law, and the doctrine of inevitable progress (as embodied in the theory of the market and the theory of the sovereign People) are assailed as absolute doctrines of human conduct which stand in contradiction to the facts of a constantly changing society. How can the view that birth control is absolutely wrong survive in a society in which the population is growing faster than the economy? What relevance has the view that monogamous marriage

alone is absolutely right to a society in which the sexes come to be numerically unequal? In the second part of his argument Mr. Corbett defines the social philosophy which he believes our circumstances to require: here he contends that liberalism in politics, the natural ally of empiricism in philosophy, best suits the requirements of the new society; it provides a form of government strong enough to control and direct innovation, but at the same time flexible enough to respond to the succession of different challenges it is called upon to meet. There is a plea for the study of behavioural science, whose task it is to discern the major social trends and provide the intellectual medium for liberal politics; and for a political education that will communicate "the settled disposition of looking forward into the social future"³. Given systematic innovation, "all comprehensive, *a priori* theories of the social order are now beside the point. The day of speculative social doctrines is now done."⁴

The weakness of this argument is its lack of system and logical rigour. There are no careful distinctions between social theories, and social policies or prescriptions. The notion of the "appropriateness" of social ideas to circumstances obscures the distinction between the truth or falsity of a doctrine, and belief or disbelief in it. Nor does Mr. Corbett make it clear why he selects the doctrines of natural law and of progress to represent the obsolete social thought which we have inherited, while he takes liberalism to belong to the social doctrine of the new order. A case can be made for the view that all these doctrines have pre-industrial origins and are therefore equally part of our "inheritance". And if liberalism is taken to be a doctrine of the industrial period, the belief in progress has an equal claim. Because the argument is loose and slippery the following attempt to discuss it may involve misinterpretations.

We may say at once that the notion that certain social doctrines are "consistent" with a certain social order, while others are in "contradiction" to it, is false and dangerous. Social doctrines are contradicted only by other social doctrines and not by circumstances. Thus it is not the case that the doctrine of natural law was consistent with the static society of the middle ages, but in contradiction with the facts of a systematically innovating society. The doctrine of natural law is, as Mr. Corbett maintains, false: but it was just as false in the middle ages as it is now. The same error underlies the notion that liberalism is, among competing political doctrines, uniquely consistent with the facts of an innovating society. The notion that a certain political

doctrine fits "the facts" is indeed a variety of natural law doctrine: it is the claim that a certain set of recommendations is implied by "nature", rather than by the demands and interests of a particular social movement. As regards the question what types of political system are best able to initiate and direct the process of innovation, a strong case could be made out for the view that authoritarian governments have a more impressive record in this department than liberal. However, whatever case there may be for the politics of liberalism rests as much on arbitrarily chosen ethical premises as on any interpretation of "the facts".

Corbett's case rests on firmer ground when what he appears to be arguing is not that old doctrines are falsified by changing social conditions, but that in these conditions belief in the old doctrines will decline. Changing conditions do not refute *a priori* doctrines, but they may affect the history of belief in them. This we may show first by examining social prescriptions, propositions about what should be done, and secondly by examining social theories, propositions about what is the case.

Moral propositions are not implied by propositions of fact, and the validity of the doctrine of natural law is not affected by changes of circumstance: Catholics are as logically entitled to believe birth control is wrong in a society in which such an attitude leads to mass starvation, as in one in which it does not. But changes of circumstance do, Corbett shows, affect the coherence of moral propositions in a system of moral belief. In certain societies it was possible to act on the doctrine that birth control is wrong, and at the same time act on the doctrine that suffering should be relieved: but in the present condition of the world, where the relief of suffering appears to require birth control, it is no longer possible to act on both these views: one or the other belief will sooner or later have to be abandoned. Changes of circumstance do not invalidate moral principles: but they destroy the coherence of sets of moral principles by replacing situations in which it is possible to act consistently with all of them with situations in which it is necessary to choose between them. Circumstance has in fact constantly assaulted natural law doctrine in this way: with the result that it has had constantly to revise its content. However, in the future, changes of circumstance will be so rapid that the adherent of natural law will be unable to adapt his doctrine to them; nor will he be able, Mr. Corbett believes, to take command of the process of innovation and so direct it as to prevent it clashing with his principles. Moreover, the sort of vast changes that made slav-

ery acceptable to the torch-bearer of natural law in one century, but unacceptable to his successor in succeeding centuries, will be compressed into the life-span of an individual: the perception of the relativity of moral principles, which was previously the peculiar experience of the historian and the anthropologist, will become the familiar experience of every man. There are good grounds, then, for Corbett's contention that not merely will faith weaken in particular manifestations of natural law doctrine, but that faith in absolute principles of morality as such will fall a victim to the innovating society.

What will be the fate of propositions about what is the case, the typical propositions of the social sciences? It has always been the lot of scientific hypotheses to be revised or discarded in the light of advances in knowledge. Under conditions of change, however, established hypotheses become inadequate for a further reason: not because our knowledge of social life has increased, but because social life has itself changed: what was correctly believed to be the case comes no longer to be the case. Descriptions of economic life in terms of the distribution of scarce resources between unlimited ends may become inadequate in "the affluent society". Descriptions of international politics in terms of a system of pressures in which the pressure one Power is able to exert upon another is gauged by its relative capacity to wage or threaten war, may have little relevance to a system of international politics in which a permanent military stalemate has neutralised war as an instrument of policy. Social changes of this kind, however, do not falsify existing hypotheses: they merely show them to have been conditional. If, in the last example, descriptions true of past systems of international politics prove not to be true of the future, they will not thereby have been falsified: it will merely have been learnt that they are true only of systems of international politics having military technologies of a certain sort. The variation of the conditions of social life in systematic innovation thus produces not the rejection, but the refinement of bodies of social knowledge; indeed if history is the laboratory of the social sciences, then systematic innovation must be seen to be a development favourable to the advance of social knowledge, for the faster the movement of change, the more thorough the elimination of the variables will be. The sort of propositions which systematic innovation is likely to undermine are of course not those which properly belong to empirical social studies, but those which purport to describe the social world in terms of logical certainty, "laws of history" and the like. While propositions which are no more than conditional hypotheses

are refined and illuminated by changes in actual conditions, propositions about what is so unconditionally are undermined by them.

Systematic innovation, then, is likely to hasten the decay of belief in *a priori* social doctrine, whether it purports to prescribe for society or to describe it. It is therefore curious that Corbett combines with this epitaph for such doctrine, a manifesto for philosophical revolt against it. Why is it especially necessary to attack such doctrines now? Given Mr. Corbett's ethical and political premises, it was pre-industrial society to which *a priori* doctrines were least "appropriate": it was then that the status of these doctrines as orthodoxies impeded the development of those modern innovating tendencies that he values so highly. What systematic innovation now requires of the student of society is not that he should flog the dead horse of a *a priori* theory, but that he should address himself in an empirical way to the actual consequences of innovation. This enterprise will have to be carried on largely by each social discipline in the light of the special problems which innovation raises for its own subject-matter. But innovation clearly raises problems which are common to many social disciplines, and which therefore call for general theoretical study⁵: the problem of predicting a future which, because it has become more uncertain, it has become more important to predict; the problem of obsolescence; the problem of planning for an unlimited variety of contingencies; the problem of the effect on present behaviour of increasing uncertainty about the future — to name some of the subdivisions of such a study. The particular study most sensitive to innovation is perhaps military science: on the one hand this subject has long been in the grip of an inflexibly conservative style of thinking, and on the other, the innovating power of science and technology is concentrated more on this field than any other: so many and so different are the possible strategic futures, and so difficult is it to decide for which or how many among them it is best to plan, that strategic thinking is driven into a kind of intellectual apoplexy. In this and other respects military science may provide a prototype of the fate of other disciplines. The general problem of innovation must be studied empirically: but this is not to say that such study is merely a matter of gathering facts about it, and not a matter of thinking about them: it is eminently suitable for "social speculation" in the best sense of that term. Mr. Corbett's achievement is to have made a contribution of originality and elegance to such a study.

Hedley Bull

London School of Economics.

NOTES

¹ J. P. Corbett: *Europe and the Social Order*, A. W. Sythoff-Leyden 1959.

² As might occur, e.g. if enough people read J. K. Galbraith's *The Affluent Society*.

³ P. 175.

⁴ P. 188.

⁵ Such a study has been suggested and baptised ("Kainurgics") by Arthur Lee Burns: "The International Consequences of Expecting Surprise" *World Politics* July 1958. See also G. L. S. Shackle: "The Logic of Surprise", chapter IV of his *Uncertainty in Economics*, 1955.

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ABSTRACTS

From "Philosophy of Science"

Vol. 27, No. 1, January 1960

On ostensive definitions, JANITA KOTARBINSKA. The first part deals with the problem of the external form of ostensive definition. It is concluded that the definition statement is not complete. The proper form of this statement is not a sentence, but a sentential function, namely a sentential function of the type: " $\text{II}x [N(x) = x \text{ is in the respect } R \text{ and in the degree } D \text{ such as } A, B \dots \text{ and not such as } K, L \dots]$ " where "N" stands for the term being defined.

Thus the ostensive definition informs about the criteria of applicability of the defined term in a partial way only, and the rest must be supplied by the addressee for whom the given definition was destined.

In the second part the conditions are analysed on which depends the possibility of solving that problem, and consequently the conditions on which depend the informational value and the efficacy of ostensive definition. The concluding remarks deal with the properties of the terms introduced by the ostensive method.

Some problems of causal interpretation of statistical relationships, STEFAN NOWAK. In the following paper an attempt will be made to analyse the statistical relationships between variables as the functions of causal relations existing between them. Our basic assumption here is that statistical relationships between traits, events, or characteristics of objects, may be logically derived from the pattern of their mutual causal connections, if this pattern is described by appropriate concepts and with sufficient precision.

The first part of the paper presents basic concepts, which according to the author's view may serve for the description of different patterns of causal relations in such a way, that statistical relationships corresponding to each pattern may be derived. It gives also examples of such a derivation for some less complicated cases. The second part of the paper is an attempt at application of the proposed method to the understanding and critical consideration of some standard techniques of statistical analysis, especially those mostly used in social sciences.

Duration and the specious present, GUSTAV BERGMANN.

Scientific reasoning and the summum bonum, W. E. SCHLARETZKI. C. S. Peirce argued that inductive reasoning and probability judgements are adequately secure only in the indefinitely long run, and that therefore it is illogical to employ these modes of inference unless one's chief devotion is to the interests of an ideal community of all rational beings, past, present, and future. He thought of this devotion as a "social sentiment", involving self-sacrifice. An examination of his argument shows that the attitude presupposed by his conceptions of induction and probability is in fact not self-sacrificial and is social only in a very special sense. Furthermore, it

seems doubtful that this attitude is characteristic of practicing scientists; and this is a reason for questioning Peirce's analysis of induction and probability.

Pragmatism and the science of Behavior, ROBERT W. BURNS. Many pragmatic philosophers insist that causality in human behavior is to be explained by psychological field theorism rather than by modern behaviorism. This paper attempts to demonstrate (1) that pragmatists often support one aspect of an untenable disjunction in psychological theory, (2) that the asserted disjunction is but a methodological distinction, and (3) that the causal order in human behavior is most likely to be profitable described, predicted, or explained by the methods of modern behaviorism.

The Duhemian Dargument, ADOLF GRÜNBAUM. This paper offers a refutation of P. Duhem's thesis that the *falsifiability* of an isolated empirical hypothesis H as an *explanans* is *unavoidably inconclusive*. Its central contentions are the following:

1. No general features of the logic of falsifiability can assure, for every isolated empirical hypothesis H and independently of the domain to which it pertains, that H can always be preserved as an *explanans* of any empirical findings O whatever by some modification of the auxiliary assumptions A in conjunction with which H functions as an *explanans*. For Duhem *cannot* guarantee on any general logical grounds the deducibility of O from an *explanans* constituted by the conjunction of H and some revised *non-trivial* version R of A: the existence of the required set R of collateral assumptions must be demonstrated for each particular case.
2. The categorical form of the Duhemian thesis is not only a *non-sequitur* but actually false. This is shown by adducing the testing of physical geometry as a counterexample to Duhem in the form of a rebuttal to A. Einstein's geometrical articulation of Duhem's thesis.
3. The possibility of a quasi *a priori* choice of a physical geometry in the sense of Duhem must be clearly *distinguished* from the feasibility of a conventional adoption of such a geometry in the sense of H. Poincaré. And the legitimacy of the latter cannot be invoked to save the Duhemian thesis from refutation by the foregoing considerations.

The problem of a logical theory of belief statements, NICHOLAS RESCHER. It is shown that the logical theory of belief statements must be prepared to take into account relationships among statements which are subtler and more delicate than is requisite in other contexts. It is necessary here to draw distinctions (of a modal and semantical character) which the standard assertory logic can ignore with impunity. This is due to the fact that it is entirely possible to be in ignorance of various logical relationships (eg., entailment, equivalence, etc.) that in fact obtain among believed statements, and so, for example to believe the premisses of a valid deductive argument, and yet disbelieve the conclusion which follows from them. The implications of such difficulties for the logic of belief statements are examined.

Vol. 27, No. 2, April 1960

On the theory of measurement in quantum mechanical systems, LOYAL DURAND III. This paper is concerned with the description of the process of measurement within the context of a quantum theory of the physical world. It is noted that quantum mecha-

nics permits a quasi-classical description (classical in the limited sense implied by the correspondence principle of Bohr) of those macroscopic phenomena in terms of which the observer forms his perceptions. Thus, the process of measurement in quantum mechanics can be understood on the quasi-classical level by transcribing from the strictly classical observables of Newtonian physics to their quasi-classical counterparts the known rules for the measurements of the former. The remaining physical problem is the delineation of the circumstances in which the correlation of a peculiarly quantum mechanical observable A with a classically measurable observable B can result in a significant measurement of A. This is undertaken within the context of quantum theory. The resulting clarification of the process of measurement has important implications relative to the philosophic interpretation of quantum mechanics.

Psychological research and Humean problems, SIRI NAESS, ARNE NAESS. In this article the question is raised whether philosophers, studying Humean problems, might profit from the empirical findings of contemporary psychology. A text from Hume's *Treatise of Human Nature* is analyzed in an attempt to find out (1) whether his problems are open to empirical testing. Each sentence in the text is classified into normative, declarative, analytic and synthetic. A prevalence of declarative, synthetic sentences is found. Further, the question is examined (2) whether contemporary empirical psychology has contributed to the testing of Hume's hypotheses. The answer is affirmative for some of the statements, and it is suggested that philosophical discussions around these problems should not be carried out as if psychological research were irrelevant.

On the analysis of history and the interdependence of the social sciences, FRANKLIN M. FISHER. The views of some historians and philosophers of history as to the possibility of fruitful historical generalization seem at odds with the underlying methodology of the other social sciences. A formal model of the world historical process is here presented within which this apparent contradiction is seen to be resolvable in terms of modern theories of probability and stochastic processes. This is done by giving rigorous form to procedures and statements in the social sciences. A formal treatment of the dependence of an investigation in one discipline on previous studies both in that area and in other social and natural sciences then follows naturally.

Do stimuli elicit behavior? — A study in the logical foundations of behavioristics, WILLIAM W. ROZEBOOM. It has become customary in modern behavioristics to speak of stimuli as though they elicit responses from organisms. But logically this is absurd, for analysis of the grammatical roles of stimulus and response concepts shows that stimuli and responses differ in logical type from causes and effects. The "S elicits R" formula thus stands revealed as elliptical for a more complicated form of assertion. The trouble with this ellipsis, however, is that by suppressing vital components of formal structure in behavioral principles, it has led to gratuitous assumptions about the environmental antecedents of behavior and seriously undermined the ability of behavior theory to assimilate the "higher mental processes."

The action of mind on body, DAVID RANDALL LUCE. Terminology and symbolism are introduced, which facilitate the precise statement of propositions concerning

the action of mind on body. The minimal meaning of "the action of mind on body" is contrasted with some of the more radical interactionistic positions to be found in the literature. These more radical positions are defined in precise formulations. It is noted that radical interactionism, or "exceptionalism" as it is here called, is a contingent, empirically-decidable issue which is quite independent of metaphysical views regarding "mind" and "matter." For that very reason it should not be the object of special philosophic concern.

CONTEXTUAL IMPLICATION*

by

Isabel C. Hungerland

In this essay, I have rejected the inductive interpretation of the paradigm of contextual implication (to say "p" is to imply that one believes that *p*) and proposed in its stead an explicatory model according to which a speaker in making a statement contextually implies whatever one is entitled to infer on the basis of the presumption that his act of stating is normal. In developing this model, I show how contextual implication depends on three distinct matters: a stating context, presumptions of normality, and rules for the correct use of expressions.

The concept of contextual implication has figured prominently in recent writings on ethics. Nowell-Smith makes it his chief instrument for exposing the errors of traditional views, and Paul Edwards relies heavily on it in working out his own, unemotive version of an Emotive Theory of ethics. But what contextual implication is, remains obscure. (I hope to show that this is an understatement.) To be sure, we all know the formula: by saying *p*, a man contextually implies *q*. This special sort of implication is, characteristically, attributed to agents, or acts of agents, rather than to statements or propositions. We also know that the relation between *p* and *q* in contextual implication is such that *p* and not-*q* is not a contradiction, but to say "p and not-q" is to perform an odd or absurd act of stating. The paradigm of this oddity, derived from G. E. Moore's writing on the subject, is "He has gone out, but I don't believe it." A man, then, by saying "He has gone out" (in a statement-making context) implies that he believes that the man in question has gone out. What is contextually implied is neither asserted by the speaker nor is it entailed by anything he asserts.

* This is a considerably changed and expanded version of a paper presented in April, 1959, at the Philosophy Colloquium, University of California, Berkeley. The comments of Professor Benson Mates, David Rynin, David Shwayder, Herman Tennessen, and Sidney Zink have been extremely helpful to me in revising and expanding the first version.

These features serve, clearly enough, to distinguish contextual implication from entailment. But the category still lacks circumscription. Will a variety of examples, a citing of cases, help here? There are plenty on hand from recent writing in philosophy. The difficulty is that when we survey these examples, these cases, it begins to be clear that some sorting out is sadly needed. Let us take a look.

Alongside Moore's paradigm example are usually placed Strawson's examples of implied presupposed statements. "To say 'The king of France is wise' is, in some sense of 'imply' to *imply* that there is a king of France."¹ The relation of presupposition between statements like "The such-and-such is P (or is not P)" and "There is a such-and-such" depends, according to Strawson, on rules or conventions for the use of uniquely referring expressions, like "the such-and-such." These rules belong to a set of conventions for referring, which are essential to *any* natural language. From these relatively abstract matters of logic, we move on in our examples to conventions that relate in part to ethics. In saying "I know it is so," I imply that I have confidence that it is so, that I am certain. In a somewhat similar way when I say "I promise to do so" I imply that I fully intend to do so. "You are prohibited from saying 'I know it is so, but I may be wrong,' just as you are prohibited from saying 'I promise I will, but I may fail.'"² A final group of examples relate, not so much to ethics as to manners and such matters as not being a bore. According to Nowell-Smith, the most important of the "rules of contextual implication is: 'What a speaker says may be assumed to be relevant to the interests of the audience.'"³ This rule, we are told, is the most frequently broken. "Bores are more common than liars or careless talkers." In its application to ethics, the rule yields the result that "... to give advice implies a belief that the advice is relevant to the hearer's problem." The rule, then, will, according to Nowell-Smith, show us that advice is practical from the start. He concludes, "This rule is, therefore, more than a rule of good manners; or rather it shows how, in matters of ordinary language, rules of good manners shade into logical rules."

What a range of rules (or "conventions," if that is a more embracing term)! All the way from the nature of statements and the rules of reference to the rule "Don't be a bore and talk off the point," — all the way from what is culture-and-language free to what is culture-and-language bound. This is why a formula like the following, which can be stretched to cover the range, is too vague and ambiguous to

be of use. Urmson writes, "The word 'implies' is being used in such a way that if there is a convention that X will be done in circumstances Y, a man implies that situation Y holds if he does X."⁴ The formula does distinguish contextual implication from entailment, but "convention" covers things that need sorting out.

There are two other accounts of contextual implication which it will be instructive to examine. One, by Paul Edwards, is precise and unambiguous, and clearly unsatisfactory. The other, by Nowell-Smith, is confused and when clear, inconsistent, but suggestive of more plausible notions. Both philosophers, however, are led to make assertions about our use of "good" and "nice" that are alike in being patently false. Both also begin their account with the paradigm: To say "He has gone out" is to contextually imply that one believes he has gone out. But Edwards,⁵ (who prefers the word "express" to "imply") follows Moore further in wrongly interpreting contextual implication (expression) as an instance of the relation between inductive evidence and conclusion.⁶ Thus, Edwards tells us that an utterance expresses "anything which it is possible to infer" concerning the speaker on the basis of his utterance. He makes it plain that he takes the inference in question to be inductive, and speaks of what is *probable* in view of what is known concerning the relevant factors in the context of utterance.

The inductive interpretation is unsatisfactory for several reasons. First, it is counter-intuitive. "He has gone out, but I don't believe it" seems to be puzzling in a way in which an exception to a well-established generalization is not. Suppose that, in a given language community, a certain expression "S" (for example, "He is a fool") is much more frequently uttered when the speaker has attitude A (dislike for the person called "a fool") than when the speaker does not have attitude A. Would "S, but I do not have attitude A" be a logically odd thing to say in this community? Clearly not. One of the regular uses of "but" is to introduce the description of an attitude of a speaker which is not the one that might reasonably be expected on the basis of what has been said in a certain context. (I am not denying that there may be a perfectly good inductive sense of "implies." I am only denying that an inductive sense is the one at issue in the paradigm.)

Here, we should note that the absurdity of the hypothetical act in the paradigm depends on the assumption that the words of the sentence "He has gone out, but I don't believe it" are being employed in a standard way (that is according to the rules given in English dic-

tionaries and grammars) and that there are no special or peculiar circumstances of communication. There are a number of non-standard ways of employing the words of the sentence for any one of which the absurdity disappears. For example, "I don't believe it" might be used as interchangeable with "It's difficult for me to believe it, though I do believe it." And there are a number of special circumstances of communication in which the employment of the sentence would not be absurd; for example, the second component sentence might be an aside. Accordingly, when one offers "He has gone out, but I don't believe it" as a paradigm of logical oddity, one is not saying that every instance of uttering the sentence would be an instance of a logically odd or absurd act. Analogously, if one gives "He has gone out and he has not gone out" as an example of formal self-contradiction, one is not saying that every instance of uttering the sentence is an instance of self-contradiction. Our ordinary discourse contains quite often sentences of the form *p* and *not-p* which we employ and take quite readily in sensible non-standard ways, for example, "It is and it isn't".

A second reason for rejecting the inductive interpretation of contextual implication is that it rests on what may well be a false generalization, namely, that people, in general, believe what they say. I, for one, am not willing to accept as true, without further investigation, the proposition that the frequency of lying in our culture is significantly less than that of truth-telling or even that it *is* less.

Finally, it would be foolish to attempt to establish the frequency of lying without classifying situations into those in which the speaker has strong reason to lie, those in which he has not very much reason, and so on. No doubt the frequency of lying would vary (except for pathological cases) according to these classes of situations. On the inductive interpretation, there should be a corresponding variation in what is contextually implied. But this consequence is not in harmony with any of the accounts of contextual implication.

Wrong as the inductive interpretation is, there are facts which make it tempting. Any adequate explanation of contextual implication will have to take these facts into account. Let us take a brief look at them.

We often make inferences about attitudes and states of mind of speakers and, in particular, about whether or not a speaker believes what he says. These inferences are made on the basis of what is said and by whom and in what manner and in what situation. We speak, in this connection, of good or bad "evidence" for our inferences, and

we employ "probable", "improbable," and "certain" for our conclusions. These inferences are rightly classed as inductive. There may be, as in the use of the lie detector, a good deal of theory and a good deal of experimental evidence behind our inferences. Or, as in daily life, we may become "pretty sure" a man is lying without being able to give a clearly detailed account of the basis for our conclusion. "Something about the expression of his eyes and his tone of voice made me pretty sure he didn't believe what he said."

In due course, I shall return to these facts. For the moment, I wish to show how Edwards' clear, but mistaken account of contextual implication (expression) leads him to assert a patent falsity about our use of evaluative expressions.

Edwards tells us that just as a non-evaluative statement expresses (implies) a belief, so an evaluative statement or judgment expresses (implies) an attitude. Now, an attitude (as distinguished from liking, preferring, willing, and so on) is defined as having the following three components: "(1) a feeling and tendency to act in a certain way towards a certain class of objects, (2) a disposition to encourage others to feel and act the same way, and (3) a tendency to be pleased if others do feel and act likewise."⁷ Then, the evaluative terms "nice" and "good" are said to *always* express (presumably on *all* occasions of their serious use) a positive attitude on the part of the speaker towards the thing evaluated. But surely this is a very dubious generalization about the psychological disposition of speakers! I know a number of people who would (to take the same sort of example given by Edwards in his account of attitude) evaluate progressive jazz as a good kind of jazz without having the slightest tendency to seek it out, or any very strong feelings for or against it, and without having the slightest tendency to encourage others to hear it, like it, or judge it to be good. In brief, it is not *always* probable that people who utter sentences of the form *X is good* (in a context of evaluation) have the psychological dispositions listed in Edwards' definition of attitude. Or, to put the matter another way, it is obviously false that the utterance of a sentence of the form *X is good* is *always* good evidence that the speaker has the dispositions in question.

Nowell-Smith's account of contextual implication lacks the clarity of Edwards'. "I shall say," writes Nowell-Smith, "that a statement *p* contextually implies *q* if anyone who knew the normal conventions of the language would be entitled to infer *q* from *p* in the context in which they occur. Logical implications are a subclass of contextual impli-

cations, since if p logically implies q , we are entitled to infer q from p in any context whatever.”⁸ When p contextually implies q , the assertion of p and the denial of q results not in formal contradiction, but in “logical oddity.” The example given is “It is raining, but I don’t believe it is.”

Unfortunately, this rough, but promising account is cancelled when Nowell-Smith tells us that the rules of contextual implication are not, like the rules of logical implication, “rigid.” “They *can* all be broken without contradiction or absurdity.”⁹ We are then given as Rule 1, “When a speaker uses a sentence to make a statement, it is contextually implied that he believes it to be true.” Lying and play-acting are said to be examples of the breaking of this “often broken” rule. But play-acting is not a stating context¹⁰ — in play-acting, we don’t use sentences to make statements, so the rule isn’t broken, it just doesn’t apply. And how does the liar “break” the rule? Doesn’t he depend on it in order to deceive us?

It is no wonder that Nowell-Smith derives a number of false conclusions from his confused account of contextual implication. I shall mention only one. We are told that when, in the context of choice, a man says, “This is good” rather than “I like this,” he contextually implies that the object conforms to certain standards which are generally accepted.¹¹ This statement is patently false. When I employ standards which are not generally accepted, I am called upon, if I wish to communicate fully with an audience that doesn’t know what my standards are, to say what they are. I am not by any rule of logic, manners, or propriety, called upon to say “I like it” instead of “This is good.” Indeed, I know a number of persons who would be humiliated to discover that they were employing generally accepted standards when they pronounced something “good.”

We have now seen that an account of contextual implication in terms of inductive evidence will not work and that Nowell-Smith’s notion of often broken rules for implication is too confused to be of use. Where, then, shall we look for an explanation of the implication between saying “ p ” and believing that p , or, what amounts to the same, of the logical oddity of “ p and I don’t believe that p ”? Clearly, there is some kind of bond between our concept of what it is to make a statement and our concept of belief as a propositional attitude. Can we find a relation of entailment between statements about statements and statements about beliefs? There are two somewhat different accounts along these lines. Neither is detailed enough to answer the

questions we have asked and neither is quite satisfactory as it stands. It will pay, however, to look at them.

C. K. Grant bases his explanation of the logical absurdity of making statements like "It is raining, but I don't believe it is" on what he calls "... an *a priori* truth deriving from the logical grammar of 'assert' and cognate verbs such as 'state'."¹² This truth is: "X asserts *p*" (or states that *p*) entails "X believes *p* or is pretending to." Part of this logical grammar is disclosed "by the fact that although it is possible to pretend to believe, it is logically impossible to pretend to assert."

Now, just how *a priori* are these alleged "truths"? To be sure, "pretend to assert" sounds a little odd, but not outrageous, (might not an actor "pretend to assert"?) Grant's "truth," however, does give an interpretation of the paradigm of contextual implication which has *prima facie* plausibility. His account calls attention to the fact that to make a statement is one of the conventional ways in which we communicate, or pretend to communicate, our beliefs. Accordingly, when we make a statement, we may be said to purport, profess, or claim to believe what we say. "To purport that ... by doing X" is one of the dictionary meanings of "imply." This interpretation provides an explanation both for the logical oddity of saying "*p*, but I don't believe that *p*" and for the failure of the inductive interpretation. If an agent should perform action X and in so doing purport that ... and then deny what is purported, his performance would be absurd or pointless. Also, to recognize an action as purporting or professing something is not to make inductive inferences.

This interpretation of "contextually implies" as "purports," plausible as it is, cannot explain satisfactorily the inference correlate of contextual implication. Why should we be "entitled to infer" or "entitled to assume" or why should "the presumption be" that what an act or agent purports is true? But these phrases are regularly employed in talking about the inference correlate of contextual implication.

Grant's account, then, does not go far enough, but it will be useful at this point to note that his "truth" is in conformity with our use of "assertion" (or "statement") when we say to the liar, "You deliberately made a false statement," or "You deliberately asserted a falsehood." The legal language connected with perjury exhibits the same use of "assertion" and "statement".

However, there seems to be a tendency for us to say, not, perhaps that the liar pretended to assert (or state), but that he didn't make a

genuine (or *normal*) statement, that he didn't *really* assert. Lewy points to this tendency in discussing conditions which some philosophers have regarded as necessary for assertion, "To state this [that A should either believe *p* or know *p*] as a necessary condition is wrong because one can certainly assert *p* and be lying. . . . We couldn't lie at all *without* asserting something or other. On the other hand the temptation to state this condition as a necessary one is, I think, excusable. For there is, I think, a tendency to say that A didn't really assert *p*, if he was lying."¹³

Clearly, if we interpret the "really" or "real" in our statements about the liar's stating or statements as the "real" that is opposed in meaning to "false," or "pretended", there is an inconsistency in our ways of talking about liars. For where "real S" is opposed to "fake S," the assertion that X is not a real S is equivalent (with some complications which can be disregarded here) to the withholding of the descriptive terms "S" from X. But in describing what the liar has done, we say that he deliberately made a false statement, with intention to deceive. We insist on attributing the descriptive terms "making a statement" to what he has done.

There are two rather obvious ways of explaining the inconsistency as apparent only. (One "explanation" of the inconsistency, of course, is that we *are* inconsistent in our talk.) Neither explanation is, I believe, wrong, but neither is adequate, I also believe, to explain fully the linguistic phenomena in question. (Let us remember here that there may not be just *one*, but several, reinforcing reasons for a linguistic use.)

The first explanation would make of the use of "really" in "the liar doesn't *really* state," an example of a transferred epithet, comparable to the transferred epithet in metonymy or prolepsis. One ingredient in the liar's act of stating is pretended, is not real, namely, belief. Pretended belief is compatible with making a statement, but, by a kind of figure of speech, the faked character of the belief is transferred to the whole act.

A second explanation would interpret the "real" or "really" in question as indicating, not the distinction between not fake, and fake, but the distinction between a logically primary and a logically secondary use of a linguistic expression. When a use is logically primary, we cannot engage in other uses, knowing what we are doing, without being familiar with the first. Nor can we describe secondary uses without at least some implicit reference to the primary one. For example, the use of "good" to mean "what people generally call good" is logi-

cally secondary to another use of good. (Examples of this sort, be it noted, do not involve pretense of any sort.) Examples of a different character, though belonging in the same general category are: the actor's use of "Something is rotten in the state of Denmark" is logically secondary to a statement use; the liar's use of a statement-form sentence is logically secondary to the truthful use. The last two examples do involve pretense; but as the first example shows, it is the logical relation, not the pretense, that inclines us, at times, to call the logically primary use, the "real" (also, the "normal") use.

This explanation has no direct relevance to the nature of contextual implication. There is no reason why we should be "entitled to infer" or "entitled to assume" or "presume" (the language usually employed in talking about the inference correlate of contextual implication) that people in saying "p" are exhibiting the logically primary use of "p". Perhaps, of course, the explanation of the apparent inconsistency has no bearing on "contextually implies." I think, however, that there is a plausible explanation of the apparent inconsistency which illuminates the nature of contextual implication. A clue to this explanation is buried in the following remarks by Strawson:

"When a man says 'p' in an ordinary statement-making context . . . his hearers are entitled to assume that he believes that *p* . . . *This is a tautology.*"¹⁴ Unfortunately, just what kind of tautology is not revealed. But clearly, the statement cannot be exhibited as an exemplification of one of the truths of logic. Strawson adds to his cryptic remarks, "If he says 'p' in such circumstances, but does not believe that *p*, there are at least two possibilities: he may be using language incorrectly, or he may be intending to deceive. We may . . . if we choose, count the second as a special case of the first." The choice would be an odd one, linguistically, but let us disregard this. What I wish to point out, at the moment, is that the following rough formalization of Strawson's two remarks, though suggested in the context, will not do.

1. "X in an ordinary statement-making context says 'p'" entails "X believes that *p*."

2. "X in an ordinary statement-making context says 'p' and X does not believe that *p*" entails "Either X is using language incorrectly or is intending to deceive."

The first member of the second entailment is, according to the first entailment, a contradiction, and a contradiction entails any statement whatsoever.

At this point, I wish to forsake exegesis for explication. Whether

my explication is one that Strawson or Grant had in mind, I do not know. But I start at any rate in agreement with them that lurking in the background of the paradox of "He has gone out, but I don't believe it," is a connection between the concept of making a statement and the concept of belief.

Some reflections on method will be in order here. In the following explication I shall have occasion to talk about certain linguistic uses in ordinary language. When I do this, I shall endeavour to put what I have to say in checkable, if not checked form. Also, I shall try not to take the concepts embedded in ordinary English and ordinary allied languages as the limits of my or anyone else's world. The important descriptive task of philosophy is bound to the study of ordinary languages. But this task should not, I think, be conceived as replacing philosophy's ancient tasks of systematizing, reforming, and speculating. The descriptive task, well done, should destroy only philosophy in her more repulsive moments of insane reform, crazy system, and obscure speculation. For example, let us grant that the writings of at least some of the German Idealists on Freedom represent philosophy at her most unattractive and that the study of the ordinary use of "freely" can show us that "freedom is not a name of a characteristic of actions but a dimension in which actions are assessed."¹⁵ But much philosophical talk on Freedom can be plausibly interpreted as an attempt to assess the dimension itself. Should the dimension be scrapped altogether or radically changed in some way? (Indeed, it has, since Freud, radically changed shape.) If philosophers ignore such questions they will find themselves confined to a very limited linguistic archeology, a fascinating and useful study, but not all that philosophy can and should do.

One last reflection on method. When I consider ordinary language uses, I shall often refer to a principle which I here christen "The Principle of attending to the Circumstances of Communication." Among "Circumstances of Communication" I include the intents and purposes of speakers and hearers and any other, not purely physical circumstances, that affect communication. Austin's formula for proceeding from ordinary language, "by examining *what we should say when*" is quite all right in the context in which it occurs.¹⁶ That context makes plain that the circumstances of communication for which the formula is employed are those of excusing oneself (or not excusing another). Under these circumstances, I can know pretty well whether I would say "I did X *by accident*" or "I did X *by mistake*" if you describe in

detail just how I am supposed to have done X. (Indeed the concepts of accident and mistake were invented for the context in question.) But if we abstract the "what we should say when" formula from its context and apply it without taking account of the circumstances of communication, we may (to take an example from Strawson's work) make the mistake of supposing that under *all* circumstances of communication, we refuse to assign a truth-value to statements like "The such-and-such is P" when "There is a such-and-such" is false. Also, we may neglect or forget the fact that in ordinary language we sometimes use "believes" intentionally (that is, the concept of what a man believes is bound to the sense of the words he employs in expressing his belief), sometimes extensionally (that is, the concept of belief is not so bound), and that these different uses may not be inconsistent, but indicative of the subtle adjustments of ordinary discourse to its circumstances. For example, there are circumstances in which I would *not* say that my gardener, an illiterate man from the South, "believes that the *Nandina Domestica* should be moved," and there are circumstances in which I would say it. The former circumstances would be those in which I might suggest to a hearer that my gardener employed and understood the Latin name and, for some reason or other, it was important not to suggest this. The latter circumstances would be those in which the employment of the Latin name would insure unambiguous reference for the hearer or the wrong suggestion would not arise, or it wouldn't matter whether or not it did. Mates provides a nice example of an extensional use of "believes"; "That gullible juror actually believes that the murderer had nothing to do with the crime."¹⁷ Here, "gullible" and "actually" will suppress wrong suggestions and the use of "murderer," without misleading, gives an effective rhetorical emphasis to "gullible." However, it is easy enough to imagine circumstances in which it would be appropriate and effective to use "believes" in the same sentence intentionally. For example, the juror might have said, "The real cause of the crime is the murderer's social environment — he was the helpless victim of an unfortunate childhood and had nothing really to do with the crime" and the speaker of the sentence in question might wish to convey his opinion of the sort of people who say such things. Perhaps I should add here that I do not suppose that a double use in ordinary language *always* indicates subtle adjustment to circumstances of communication rather than just plain lack of clarity or consistency. The degree of muddle in ordinary language will correspond to the degree of muddle-

headedness of the ordinary man, and this is considerable. My point is, rather, that a double use is not *always* indicative of defect.

The circumstances of communication, we should remember, not only affect *what we should say when*, but they also may be referred to in our talk about our talk. "Intention to deceive" is part of the definition of lying.

It is time now for a preliminary sketch of how I propose to explain contextual implication. For reasons which will emerge, I shall not attempt to formalize the explication, that is, formulate it in statements of entailment.¹⁸ I shall instead elucidate, informally, certain uses of "normal", "real," of "stating," "saying," and "believing." These uses are so related that, if adopted, they would render our talk about liars consistent and yield an answer to some of the puzzles about contextual implication. I shall refer to these inter-related uses as "explicatory" and as "model" uses (employing "the model" for short) for the following reasons. I believe that they have some resemblance to ways in which some people discourse on these matters. (Later, I shall indicate how empirical checking might be undertaken.) But the correspondence would depend on the discourser's interest in being systematic and consistent.

It is important to remember, particularly in the case of "normal" and "correct use of language," that the terms to be elucidated are taken as occurring on the level of ordinary language, not on the philosophical meta-level, Second-order statements (statements about statements) are involved in the model, but these are statements *within* ordinary language. The model consists in the following interrelated uses.

1. To say "p" in a stating context (that is, under conditions which fulfill the criteria for such contexts) is, for the relevant sense of "say," to state that *p*. (I shall sometimes for "to state that *p*" employ the locution, "to make the statement *p*.") Whether or not the criteria for a stating context are met is, I shall show, a matter to be established empirically, either by a simple looking to see (checking) or by an inductive procedure.

2. "Real" in "The liar does not make a real statement" is interchangeable with "normal" in the very ordinary evaluative use of these words in which they are not antonyms of "fake" and not synonyms of "usual."

An act of stating is evaluated as "normal" only if the speaker believes what he says. (The same requirement will hold for calling a stating context "normal.") The following two conditions must be ful-

filled when a man believes what he says: intention to tell the truth, that is, lack of intention to deceive, and correct use of language.¹⁹ (These two conditions, then, function as at least part of the set of criteria for the evaluative use of "normal ") Accordingly, when a man, in a stating context, says what he does not believe, his act of stating is not normal and there are (at least) two possibilities: that he is intending to deceive (lying) or that he is using language incorrectly.

The two conditions involved in believing what one says clearly fall into the category of *circumstances of communication*. The absence of either interferes with communication in its root sense of sharing beliefs and thoughts. Furthermore, the speaker is considered responsible for fulfilling both conditions, though perhaps more responsible for not lying than for using language correctly.

The two circumstances of communication in question form the content (or part of it) of what I shall call "presumptions of normality" in the act of stating. These presumptions are, I shall maintain, comparable, in the following respects, to the principle in Anglo-Saxon law that a man is presumed innocent until proved guilty. The principle is *not* inductively established — it cannot be interpreted to mean that a man charged with a legal offence is more likely to be innocent than guilty. However, the principle is not independent of inductively established frequencies. A drastic change, due perhaps to new methods of criminal investigation, in the frequency of guilt among those charged might well lead to a change in the principle or to its complete abandonment. Furthermore, the principle is closely connected with ethical considerations of fairness, of giving people the benefit of the doubt. Finally, the principle is culture-bound, but not in any ordinary sense "arbitrary" or "capricious" or "unjustifiable". The presumption of normality in the act of stating, I hope to show, might appropriately be called a "principle" of communication in our culture. The "innocence" of the speaker is, in the absence of evidence to the contrary, taken for granted in the cooperative enterprise of discourse.

Now, both the presumption of normality and the criteria for stating contexts might, in a very loose sense of the word be called "conventions." But they are, it is plain, very different sorts of things and each is also distinct from a third factor in the phenomenon of contextual implication: the linguistic rules ("conventions" in a less loose sense) that give the correct use of the expression employed by the speaker. When a man says "p" there will be rules which determine the correct employment of "p", and of constituents of "p".

I shall explain the paradigm of contextual implication ("To say 'p' is to contextually imply that one believes that *p*.") as depending in the following way on all three factors. 1. *The presence of a stating context.* The question of a man's believing what he says does not arise in a non-stating context. 2. *The presumptions of normality.* Within a stating context, the implication holds only if the presumptions are principles of communication. 3. *Rules for the correct use of an expression.* Given the presumptions of normality within a stating context, then whether belief is implied when a man says "p" will be determined by rules for the correct use of "p." (For example, is "p" correctly used to ask a question, not to make a statement?) Further, if "p" is correctly employed as a statement-making sentence, then *what* it is that the speaker implies he believes will be determined by rules for the correct use of the constituents of "p".

The model just indicated might be summarized as follows: a speaker in making a statement contextually implies whatever one is entitled to infer on the basis of the presumption that his act of stating is normal.

All that is said in what follows about the making of statements should be taken to cover also, with appropriate changes, the asking of questions, the issuing of imperatives, the making of appraisals, and so on. Also, it should be noted, "context" is employed by Strawson and others to include both situation and manner of speaker. I shall sometimes consider the two factors together, sometimes, separately.²⁰

It will be convenient to elucidate the model in the following order. I shall consider first the criteria for stating contexts, then, the concept of saying something, and finally, the use of "real" and "normal" in connection with acts of stating.

Is there a simple and closed set of criteria for either stating situations or stating manner or way of speaking? The first thing to notice is that the philosophers who write about these matters indicate only negatively and by example what a stating context is. A stating situation is not play-acting, not testing a microphone, not telling a story, not practising pronunciation, and so on. A stating manner is not crossing one's fingers, not winking, not displaying an ironic tone of voice, and so on. The convenient "so on" is never further detailed. Let us pull out a few more items. To the list of non-stating situations, we might add the mixed or impure examples of the cocktail party, the love scene, and the psychoanalyst's hour. When we think of non-stating manners or ways of speaking, we should not forget the idiosyn-

crasies of individuals in using declarative sentences in the indicative mood. I know two adults who on certain occasions revert to the state of a child who does not yet distinguish between fact and fantasy, between serious planning for the future and make-believe projects. Only the intimate friends of these two people can recognize the non-stating manner that goes with their regressed state.

There is, I think, a good reason why we are ordinarily given a negative indication of what a stating context is. Once we have emerged (either as a race or as an individual) from the stage where no clear distinction is made between the serious and playful, the literal and non-literal uses of language, the stating use is the *logically* primary one. One can, of course, say truly, but trivially, that stating contexts are these in which considerations of truth and falsity enter. But the point is that there seems to be nothing in common to these contexts beyond the fact that truth and falsity are relevant to them. Accordingly, the listing of a few well-known non-stating contexts should suffice to indicate, to a sophisticated audience, the primary context.

Is there an equally good reason why the concept of a non-stating context is elucidated by example, rather than by definition? I believe so. The non-stating situations and manners listed are a heterogeneous set, exhibiting no common "essence" from which we might derive a few criteria. In calling the whole negative set "playful" or "not serious" we have to distort the usual meaning of these terms. The literary use of language is serious and is by no means in simple contrast to a stating use. "Fiction" is a narrower term than "literature." Telling a fairy tale and testing a microphone seem to have nothing in common except their being outside a stating context. And speaking in an ironic tone of voice is a very different "manner" of speaking from winking when saying something. Suppose, for example, that from the example of a speaker practising pronunciation we extract the criterion: the interest of speaker and possible audience is concentrated solely on the phonetic aspect of the words. This negative criterion will not apply to the other negative instances.

Similarly, there is no single "essence" and hence simple set of criteria in the various standard instances of stating contexts: testimony in court, discussions of the consequences of social regulations, scientific research. Also, Shakespeare has shown us, in a play, what serious use can be made of a play. In real life, Mme. de Staël provides a fine example of a performance which was both play-acting and statement. When her lover, Benjamin Constant was about to desert her for an-

other woman, she put on a production of Racine's *Andromaque*. She assigned herself the rôle of Hermione, a jilted woman, and Constant, the rôle of Pyrrhus, the jilter. The big scene of Hermione's denunciation of Pyrrhus seems to have been greatly enjoyed by everyone (the audience knew the circumstances) except, of course, Constant. I hardly need add in leaving the topic, that the criteria for stating contexts may differ widely among different cultures.

The next question to be asked is: Does our telling, whether a context is a stating one or not, have to be a matter of inductive inference, of weighing evidence? My answer is: No, but neither does our telling *have* to be always non-inferential, a matter of looking to see. When I go to the theater, I do not exercise inductive procedures to make sure that what I am witnessing is a play, and not, for instance, a murder. I recognize the building in which I am as a theater and what is going on as a play. But it is easy enough to imagine situations (the apartment with thin walls, the voice on the radio) in which one might have to weigh evidence in order to decide whether what one heard was the voice of a murderer or the voice of an actor playing a murderer.

I turn now to the concept of saying something. Our use of "say" has some odd and interesting features. In elucidating them, I shall try to avoid both the appearance and the fact of laying down arbitrary rules for proper or correct usage. I shall be reporting what, with the requisite critical interpretation, I have found from a rather careful perusal of dictionaries, and where I write "we do not say" I am referring to the sort of expression I should mark "odd" or "wrong" or "inappropriate" on a Freshman theme. Two points are important in this connection. First, not even the most trivial question of language usage can be answered by an uninformed and uncritical consulting of the dictionary. Second, my aim here in eliciting from the dictionary correct or proper use is not, as it is when I grade Freshman themes, the teaching of literary skill. My aim is to take the further philosophical step of eliciting from the elicited rules certain concepts which need to be distinguished in this inquiry.

What, then, do we find when we (critically) consult dictionaries about the use of "say" and the closely related "utter" and "state"?

We "utter" words and sentences as well as groans and the like; we do not, however, "utter" *that*. . . . We do "say" *that*. . . . Do we "say" words and sentences? If we reply *yes* on the basis of "You said a dirty word," we should remember that this locution is perhaps an illuminating survival from childhood. When we were children it was not

only human beings that "said" things to us. Our parents may have encouraged us in this notion by telling us that the cow *says* "Moo" and so on. What from a grown-up viewpoint is a metaphoric extension of "say" to "utter" is the original childhood field (and perhaps racial field) of the concept. But talking as grown-ups, "He said the sentence slowly" is not good, standard English, "spoke" or "uttered" being proper in the context.

"Say" thus has a boundary on one side which marks it off from "utter." "Utter" refers to the issuing of sounds which may or may not be the sounds of words; and when a man utters sounds which are the sounds of words, he may or may not be employing the sounds as discourse. For example, both my Chinese friend and I (who know no Chinese) can "utter" the Chinese sentence "C". (I listen to him and copy the sounds of his Cantonese or Mandarin as the case may be.) But only my friend in uttering the sentence "C" also says "C" and perhaps says *that C*. (Note, "He uttered the sentence 'You lie'" is all right, but not "'You lie,' he uttered.")

It is this employment of "says 'p'" that I am interested in. If we consult a dictionary, we find incorporated in the concept of saying, that of *expressing something in words*. This somewhat old-fashioned phrase may, not arbitrarily, I think, be interpreted as follows. To say "p" is to employ sounds as words, that is, knowingly to use sounds as some kind of discourse. (That there will be border-line cases does not make the distinction useless.) The parrot, only by metaphoric extension, says "Polly wants a cracker," the cow, "Moo" and the infant, at one stage, "Mama".

"State" gives the other boundary side of "say". One can "say" *that* ... and "state" *that* ... But, though the actor *says* "Something is rotten in the state of Denmark," does he *say that* something is rotten in the state of Denmark? We hesitate, I believe, and our hesitation is instructive. "State that" can be substituted for "said that" only where a statement context is understood, for example, "You lied. You *said that* ... and you knew it wasn't so." But this substitution is so often legitimately made that we hesitate in the case of the actor where "state that" cannot replace "said that."

Does "*saying* 'p'" (uttering "p" with the intention of engaging in discourse of some kind) involve the correct use of "p" and its constituents? The answer to this question will make still clearer the boundaries, as I am employing it, of "say" in the model. Suppose, for example, that a student who wishes to express his belief that Onto-

geny recapitulates phylogeny utters the sentence "Ontology decapitates philology." Here words, constituents of a sentence, are incorrectly employed. Does the student say "Ontology decapitates philology"? Yes, according to the use of "say" in the model, he says this and says that Ontology . . . and, if the context is a statement one, he states that Ontology . . . (I do not wish here to neglect the Principle of the Circumstances of Communication. It is easy to imagine circumstances in which I would not employ "state" as I employ it here.)

All that I claim is, first, that the use provides a simple model, and second that it is not counter to *some* ordinary uses in *some* circumstances. For example, suppose I, speaking in a foreign language, unwittingly employ a libelous term "L". A natural way to excuse myself would be to say that I didn't mean what I said, that I didn't intend to state that X was an L, or that I didn't believe that X was an L, but not that I didn't make the statement in question. Now, if a student utters the sentence "Ontology decapitates philology" and I have reason to assume that he is not intending to deceive and also that he does not believe the statement he has made (though many might believe it), but the quite different one about Ontogeny and Phylogeny, what is my conclusion? I conclude that he is using language incorrectly, and this incorrectness of use, be it noted, is on the level of ordinary English, not on a philosophical level.

Our example has illustrated the incorrect use of a constituent of a sentence. What would be an example of an incorrect use of a sentence? It might be said that, if all the constituents of a sentence "p" are correctly employed, then "p" itself is correctly employed; the notion of a rule for the use of "p" itself is otiose. But when we are concerned with the syntactical difference between kinds of sentences — declarative, interrogatory, and so on — it is, I think, natural to speak of the uses or functions of the sentences themselves. Consider, then, the following example, A non-native speaker of English whose native language differed greatly in syntax from English might at some point in his learning the language mistake a declarative sentence with verb in the indicative mood for an interrogatory sentence. The non-native speaker, intending, for example, to ask whether Jones has gone out, says (in a stating context) "Jones has gone out." The speaker does not, in this example, believe what he says and the explanation for the anomaly is — provided other conditions of abnormality are excluded — that he is employing the sentence "Jones has gone out" incorrectly.

It will be in order now to examine the role of "normal" and "real" in the model.

Let us remind ourselves of one of the linguistic phenomena to be explained. There is, on the one hand, the fact of our saying that the liar asserted or stated *p*. Indeed, if the liar doesn't do something correctly described as stating or asserting, he isn't lying at all. On the other hand, there is a tendency to modify our description of the liar's statement by saying that it wasn't a "genuine" or "real" or perhaps "ordinary" act of stating. Sometimes, too, we are inclined to say that the liar's act of assertion isn't a "normal" one. The "normal" in my formula, it will appear, should be taken as the "normal" which is interchangeable with "real" and "genuine" in the use in question.

In dealing with the concept of a *normal* act of stating I wish expressly not to invoke Austin's principle, *No modification without aberration*. The principle, applied to the phenomena under scrutiny, would obscure our use of "ordinary" and the like in ordinary language. Its application would also lead to the conclusion, which I regard as mistaken, that when a man lies, something has gone wrong with an act (in the sense that a condition for its coming off successfully is absent) and we don't know quite what to say.

How, then, does the role of "normal" in my formula differ from Austin's use of "normal" and the like in the explanation of his principle? The passage in question reads:

When it is stated that X did A, there is a temptation to suppose that given some, indeed perhaps *any*, expression modifying the verb we shall be entitled to insert either it or its opposite or negation in our statements: ... In the great majority of cases of the use of the great majority of verbs ... such suppositions are quite unjustified. ... Only if we do the action named in some *special* way or circumstances, different from those in which such an act is naturally done (and of course both the normal and the abnormal differ according to what verb in particular is in question) is a modifying expression called for, or even in order. I sit in my chair, in the usual way ... here, it will not do to say either that I sat in it intentionally, or that I did not sit in it intentionally ...²¹

Now, in the statement of the principle, the "normal," "natural," "usual" are employed in the philosopher's extended, second-level use of these words. (I have, be it noted, no objection to this use nor to the principle *as applied* by Austin.) A normal (usual, natural) walk across a room, in Austin's sense, is just such an action from whose description

the ordinary man would withhold a modifying expression, including, of course, the modifying expressions, "normal," "usual," "natural" and "ordinary".

How do these adjectives and the corresponding adverbs behave in ordinary language? (I shall omit "naturally" from consideration as adding unneeded complication.) Let us start with some well-known facts about their behavior. "Normal" is sometimes interchangeable with "ordinary" and "usual," sometimes not. If you have been giving yawns of ten minutes duration, and finally there comes a yawn that endures only one minute, I might exclaim, "At last, a normal (usual, ordinary) yawn!" or "At last, you're yawning normally (or in the usual way)!" But, as we all know, the ordinary man (the usual specimen) is in body, decrepit; in mind, weak; in spirit, unwilling — in brief, not normal at all. And as "Snafu" tells us, the normal (usual, ordinary) way of doing things in one profession is to make a mess of it, that is to say, to fall short of some relevant norm of efficient performance.

When do we in ordinary, (not philosophic) language employ the modifiers "normally" and "normal" in the sense in which they are *not* interchangeable with "usually" and "usual"? Here again, the *No modification* principle is inapplicable. To say that we employ these modifiers only when something is fishy, would be itself a little fishy; and to say that we employ them only when something untoward happens, would be mistaken. We employ these modifiers when there is some reason to invoke a norm, standard, scale, against which to measure or evaluate something. But the reason may just as well be that things are going very right as that they are going very wrong. The doctor after your check-up may tell you "Everything is functioning very normally." The wife of a psychiatrist reported that her husband proposed to her in these words, "I love you. You act so normally!" There need, of course, be nothing in common to all the occasions when norms are invoked.

The next step in our preliminary clarification is to see the connection, on the ordinary level, between "normal" and "real" (or "genuine") and the difference in behavior between "real" when opposed to "fake" and "real" when opposed to "abnormal" or "not normal," ("real" when interchangeable with "normal").

The example of sexual deviation for which "normal" and "real" are interchangeable is illuminating, but has needless complications. Consider, then, a clergyman who makes it a practice to break, in

rotation, one of the ten commandments every day. We would, I think, be inclined to say that he wasn't a "normal" clergyman (or call him an "abnormal" one) and to vary this expression by saying that he wasn't a "real" clergyman. "Not real" here obviously does not mean the same as "bogus." To say of our systematically commandment-breaking clergyman that he isn't a normal or real clergyman calls for his being a clergyman. A clergyman who is not a real one in the sense of being a bogus one is not a special or peculiar sort of clergyman, but not a clergyman at all. The difference in function between "not real" when interchangeable with "fake" and "not real" when interchangeable with "not normal" is, plainly, this. *X is not a real (i.e. X is a fake) P* puts X outside the class of P's. (Sometimes, of course, it is difficult to decide whether the amount or degree of fakiness warrants exclusion. Then, we are not sure whether to say "X is a P" or "X is not a P.") *X is not a real (i.e., not a normal) P* measures X on some scale or against some norm appropriate to P's. The evaluating makes sense, then, only if X is indeed included in the class of P's, only if X is, in the appropriate meaning of the words, a *real* or *genuine* P.

The importance of these pretty obvious linguistic observations is this. They prevent us from confusing an unqualified withholding of a predicate P from X with the qualified application of P where the qualification is "not real." Our tendency to say that the liar's statement is a not a real or genuine act of stating can be understood now as quite consistent with the fact that in order to lie one must state or assert something. The "real" in this context can, and, I suggest, should be taken as interchangeable with "normal."

One final question and we shall be ready for an assessment of the explicatory model. In what sense can an act, rather than a statement or proposition, be said to *imply* something?

There is this very clear sense in which an act (or its larger-scale relative, an action) may be said to imply something: the description of the act (or action) as having been performed entails what the act is said to imply. Thus, "That Jones gets married implies that he was not already married" may be reformulated as "Jones got married" entails "Jones was not already married." How does this formulation apply to our model? We have seen that "X makes the statement that *p*" does *not* entail "X believes that *p*." But can we not formulate one part of the model as the following entailment? "X makes the normal statement (or normally states) that *p*" entails "X believes that *p*."

There are several reasons why it is inadvisable to formulate the model as a set of entailments.

First, the relation between the evaluative use of a term and the criteria for this use is still a moot question among philosophers. Does "X is good" when "good" is used evaluatively (as a "grading label" in Urmson's terminology) mean "X has characteristics A, B, C" where A, B, C are the criteria for evaluation? I am inclined to agree with Urmson that the relation between a grading label and its criteria is neither analytic nor synthetic, that the distinction applies only to purely descriptive uses of language, not to evaluative or grading uses. However this may be, it seems wiser not to dogmatically prejudge the issue, but to describe the relationship with a vaguer word than "entailment." "Entail" is not a term of ordinary language. One can, to be sure, find second-order statements in ordinary language that can be plausibly interpreted as statements of entailment. But, owing to the (useful) vagueness and ambiguity of rules of use in ordinary language, statements of entailment in that language are not, like the entailment statements of logic, precise and without exception. For example, not all statements of the form *X is a bachelor and X is married* are self-contradictory in ordinary language. A man caught in the complexities of the divorce laws of California, Nevada, and Mexico, might appropriately be described as a bachelor, but still married. The more a sentence formally resembles a tautology, "where men are men", "a rose is a rose," the less likely, in ordinary language, it is to be so used. Now, if we add to the difficulties of extracting entailment rules from ordinary language in fairly clear-cut cases, the difficulties attendant upon the perplexing case in question, we have needlessly complicated matters.

Last, if the model is to be in a shape that lends itself to empirical testing for conformity to ordinary use, a technical term like "entails" is not advisable. An ordinary questioner cannot understand "does 'p' entail 'q'?" until he has been given at least a brief course in logic. But when he has had this course, he is no longer an ordinary questioner. He may, alas, be well on the way to having philosophical theories about the "real" meaning of terms.

Accordingly, I suggest that the relation is question be formulated along the following lines.

What is contextually implied by an act of stating is the fulfillment of those conditions *involved* in the characterization (description, evaluative account) of the act as normal. Questions like the following

could then be put to an ordinary questioner: "If you said (stating context, of course, understood), that the act was normal, would you also say that . . . ?" or "If you evaluated (appraised, judged) the act to be normal, would you consider yourself committed to stating that . . . ?"

It should be clear now that the inference correlate of contextual implication is distinct both from inductive inference and from deduction. (Indeed, it is dubious that "inference" is an appropriate word as a correlate for "contextual implication.") In assuming that X believes *p*, on the basis of the fact that he says "*p*" in a stating context, we are neither tracing the entailments of his statement or any other statement nor inductively inferring his sincerity and correct use of language. Contextual inference (if we wish to use the word) is a matter, rather, of a communal assumption in the absence of evidence to the contrary, that, in a situation of communication, acts of stating are normal.

Thus far, my clarifications of the explicatory model have been partial only. They will do, I hope, for a rough assessment of what the model does and does not accomplish. But some questions about the model remain to be answered. It will be well here to indicate what they are and where, in the course of the argument, I plan to deal with them.

There is, first, the question of the exact nature of logical oddity. Thus far, I, along with all others who have written on the subject, have appealed to the intuition of the audience in calling a linguistic act "logically odd." But intuition needs support from discursive explanation. One wants, or *should* want to know, for example, just how stating *p* and negating the contextual implication of so doing resembles and differs from the act of stating *p* and *non-p*. I shall deal with these questions at the end of this section, when I consider once more, in conclusion, the paradigm of logical oddity, "He has gone out, but I don't believe it."

Next, my assertion that the presumptions of normality are culture-bound needs support and the question, "How are the presumptions related, on the one hand to ethical considerations, and on the other to empirical frequencies?" needs answering. Also, there is the question of how the model accommodates the fact that we often do establish inductively that a man is not lying or that he is using language correctly. I shall answer these questions shortly in connection with the assessment of the model.

Finally, there is the question of just what role linguistic conventions (or rules of use) play in determining contextual implication.

This question has already been partially, but far from adequately dealt with. In the very general case, where we consider only that a man utters a declarative sentence in the indicative mood, knowingly employing the sounds as discourse ("says 'p'"), he contextually implies that he is employing "p" correctly. Correct use here would involve not only employing the component words of any "p" correctly ("ontology," and so on, in our earlier example), but also that "p" is not mistaken for an interrogatory or imperative form, or the like. In brief, rules (or conventions) of both semantical and syntactical correctness determine what it is that we assume about a man's beliefs when, in a stating context, he says "p". Even though the vagueness and ambiguity of "rule" and "convention" are not as great in this explanation as they are in, for example, Urmson's account of contextual implication, they still, perhaps conveniently, cover a multitude of things. I shall, after appraising the model, try to indicate how some of the members of this multitude function in determining *what* belief a speaker contextually implies. For this purpose, I shall select Strawson's rules of reference and his rule concerning the presupposition of statements like "The President of the U.S. is . . . years old." Both Strawson and his critics have interpreted the rule (or convention) concerning presupposed statements as a rule of correct use for ordinary language. The critics, of course, have gleefully pointed out that the rule does not hold for all ordinary language situations and Strawson has admitted that there are more exceptions to his rule than he first thought. I shall argue that Strawson and his critics are equally mistaken in their interpretation of the rule's function and of its domain of application.

What can and what cannot be claimed for the explicatory model? I do not, in the first place, claim originality for it. In some sense (which I refuse, on this occasion, to specify) the explication offered amounts to saying pretty much the same thing that Strawson and many others have said about the relation between belief and the act of stating. But there are, I think, advantages — rhetorical, philosophical, and logical — which can be claimed for the model as compared with, for example, Strawson's account. To see what these are, let us look again at his formulation: "When a man says 'p' in an ordinary statement-making context . . . his hearers are entitled to assume that he believes that *p*." This statement, Strawson adds, "is a tautology."

An exhibition of the tautological or analytic character of the statement would have to go something like this. To say "p" in such

circumstances ("ordinary" here, note, is the philosophical "ordinary") just *is* to claim to believe that *p*; it just *is* one of the ways (and perhaps the standard way) to make that claim. And what we mean by "making a claim" is analytically connected with "entitling people to assume." To make a claim is to entitle one's audience to assume.

Should anyone, faced with this account, ask, "But why is to do *this*, to claim *that*?" he will probably be told: When there is a convention (rule) to the effect that X will only be done under circumstances Y, a man claims (implies) that Y holds if he does X. The convention (rule) just *is* the convention (rule) for using language (correctly). To ask why it is the rule (for correct use) is a foolish question, somewhat like asking why the rules of chess tell us how to play chess (correctly).

It should be noted that "using language correctly" in this formulation is not operating on the ordinary, grammar and dictionary level, but on the philosophical level. This level of linguistic operation explains, I think, why Strawson is not disinclined to the absorption of lying into correct use. On the ordinary level, such absorption would be very, very odd. Correct use (ordinary level) is pretty essential to successful lying.

The first advantage which the model has is rhetorical. It will, I believe, fall more sweetly on certain American and Norwegian ears than the Oxonian formulation, and these are ears which I should like to have lent me.

The rhetorical advantage is closely connected with certain philosophical advantages. The model, because it is phrased on the ordinary language level is empirically checkable, either by a Socratic questioner or a Norwegian questionnaire. (This feature I take to be philosophically advantageous.) I prefer a combination of the two in empirical checking on the ways in which we employ language. Each source of questions has its merits and demerits. Faced with a questionee's outlandish interpretation of questions and inconsistent answers, the questionnaire is inflexible and mute. On the other hand, the Socratic questioner may pluck from the mouth of the questionee what he, the questioner, has first put there either by tricks of rhetoric or by just plain charm. A combination of the two methods might retain the advantages and correct the faults of each. Whether one or both methods are employed, the Principle of Attending to the Circumstances of Communication should not be neglected.

Both the working out of a method of empirical checking and the checking itself lie beyond the scope of this work. I shall content

myself with noting that the following general lines of inquiry would tend to show how nearly the model resembled one or more sets of language use among ordinary people. First, an attempt would be made to get at different uses of "implies" and to isolate the use (or uses) in which implication is attributed to an agent or his act. Then, questions concerning uses of "normal" and "real" would be asked, and the way in which "normal" as an evaluative term is applied to linguistic performances investigated. Uses of "say" and allied terms would be studied. In the whole inquiry the interrelationship of terms, within a certain universe of discourse (talk about stating and believing) should not be neglected. What the empirical semanticist investigates is always, I think, a network of uses of words, not a set of independent, mutually isolated uses.

The model has also the philosophical advantage of avoiding provincialism in the way in which a man avoids being derogatorily provincial when he knows that he may not be speaking in the accents of the capital. I have said that the presumptions of normality in the act of stating are culture-bound but not arbitrary, related to but not determined by inductive frequencies, and closely connected with ethical considerations. They are not, then, to be confused with culture-free laws of logic.

There are two points to be kept in mind in examining the nature of the presumptions. First, in order to show that they *are* culture-bound, it is not necessary to prove that another culture has different presumptions. It will suffice to show the *possibility* of there being different presumptions in different cultures. In what follows, I shall both refer to anthropological investigation and appeal to the reader's imagination. But I cite the investigations more to demonstrate possibilities than to prove facts. Second, there are two different ways in which presumptions about actions might differ from our own. The criteria for normality might differ or the presumption might be that of abnormality. (I shall not explore all the logical possibilities here.) The latter possibility may appear to be much less of a cultural probability than the former, and perhaps this is so. However, the presumption, in French law, of the guilt of those charged with an offense seems, compared with the presumption of Anglo-Saxon law, something like a presumption of abnormality. The frequency of the guilty among those charged is not, I believe, significantly different in France than in those countries where innocence is presumed. Nor is there any evidence that illegal behavior is considered "normal" (evaluative use) in France.

Whatever the interpretation of the differences in question between French and Anglo-Saxon law, the possibility of different content in presumptions of normality in different cultures is one with which we are familiar. To make my point, I propose to offer first the case of a culture in which lying appears to be taken as normal, proper, right. The culture is that of the Barundi (singular, Murundi) of Urundi who speak Kirundi. Ethel Albert, who investigated the ethics, logic, and rhetoric of the Barundi, writes as follows about their attitude towards lying:

It is not maligning the Rundi to speak of their tendency to lie. They speak of it themselves with pride. Their explanations of why it is necessary to lie in self-protection form an interesting part of their view of life.²²

Telling a lie . . . is a basic cultural skill and woe to him who lacks it. . . . To some extent successful lying is valued as an art in itself, but chiefly, it is valued for its profitable results. . . . The key concept for the analysis of Urindi discourse is *ubgence* . . . The least common denominator of *ubgence* is cleverness, . . . in this sense, then, *ubgence* means intelligence, shrewdness, the ability to deceive — above all, ability to deceive. . . .²³

Because any individual is expected to protect his own position, anything [in a Court of Law] goes: he may lie, . . . contradict himself to confuse the judges . . . Here rhetoric rules and nobody objects. But the . . . judge, or elder, is present at a hearing precisely because his disinterested judgment is wanted. It is assumed that the contestants will both use rhetoric.²⁴

These anthropological observations will, I trust, suffice to suggest the possibility of a culture in which saying what one doesn't believe (with intent to deceive) is taken as a normal, proper or right linguistic act for most people, and in which normality is presumed. If this be granted, then the model is, in respect to one of the conditions of normality, culture-bound. I hasten to add that neither our concepts of the normal nor our presumptions of normality are shown to be arbitrary or capricious or inferior to other concepts and presumptions when they are shown to be not culture-free. I do not know what causes so many who come to see that certain of their concepts are culture-bound to be thereafter debilitated in their power of judgment. Perhaps it is the severity of the mental wrench required to achieve the perception of culture-binding. At any rate, there remain good reasons to prefer our criteria of normality to those of the Barundi. Asked how the deception-admiring Barundi get along, Dr. Albert replied, "Not very well."

As for our presumption that speakers are employing language correctly, it is easy enough to imagine a culture in which this presumption would not be made. Inductive frequencies may well be more influential in determining the presumption of correctness than that of lack of intention to deceive. Further, our responsibility for speaking correctly is not as great nor as clearly "moral" as our responsibility for truth-telling. But it is still part of our responsibility in using discourse to employ it so as to inform, not to mislead our audience. (Minor incorrectness, "He don't" will not mislead, but major ones, "ontology" for "ontogeny" will.) The presumption, then, that speakers are employing language correctly, is a special case of giving the other fellow the benefit of the doubt, of being fair, of presuming innocence until guilt is proven.

The question of when and how we do establish inductively that a man believes what he says can now be answered briefly. When evidence of one sort or another appears against our presumptions, we, if we are sensible, investigate, and our conclusion may, of course, be that the evidence *for* outweighs the evidence *against*. Though the man did look furtive and one could hardly believe the story he told, the lie detector test (or one of the many other ways of checking) showed him to have been (probably or certainly) telling the truth. The important point in this connection is that an inductively established belief in a speaker's veracity (or in the correctness of his use of language) is as distinct from the presumption of normality as is the post-trial inductively established belief in an accused man's innocence distinct from the initial presumption of his innocence.

The final philosophical advantage which I shall claim for the explicatory model is that it makes plainer than do other accounts the way in which linguistic rules (conventions) enter into the phenomena of contextual implication. To back up this claim, I propose to examine Strawson's doctrine of presupposition.

Strawson defines "S presupposes S'" as follows: "The truth of S' is a necessary condition of the truth or falsity of the statement that S."²⁵ (The fact that France is not now a monarchy provides the most famous illustration offered of the relation of presupposition between statements. However, the relation between *All my children are asleep* and *I have children* illustrates the relation equally well.)

There are three things about Strawson's definition which should be noted at the outset.

First, "necessary condition" and "truth or falsity of the statement

that . . ." cannot be interpreted in the ordinary (in logic) truth-functional way. Professor David Rynin has pointed out that, on this interpretation, the definition entails the paradoxical consequence that all presupposed statements are true. Rynin's demonstration (employing the symbols of the truth-functional system in the customary way) is as follows. $S > S'$ and $\neg S > S'$ but $Sv\neg S$. Therefore S' . To put the matter another way, $[\neg S' > \neg (Sv\neg S)] > S'$ is an analytic truth-functional formula in the two-valued truth-functional system.

Second, the relation defined by Strawson is *not* that of contextual implication. (Some writings on the subject blur the distinction.) The relation between the two may be indicated as follows: when S presupposes S' , a speaker, in making the statement S , contextually implies that he believes that S' . (According to the explicatory model, when S presupposes S' , a description of a speaker's stating S as a *normal act of stating*, will involve the statement that he believes S' .) Thus, if I say — in a stating context — "All my children are asleep" I contextually imply that I believe that I have children.

Third, the definition makes no mention of or reference to beliefs or intentions of speakers or hearers or to other circumstances of communication. However, the definition does have, Strawson writes ". . . the fairly obvious consequence that, where S presupposes S' , it would be incorrect (or deceitful — the cases are different) for a speaker to assert [or to deny] S unless he believed or took for granted that S' ."²⁶ Another form of the rule for the correct employment of S where S presupposes S' appears in Strawson's discussions of the matter. This other form of rule follows from the relation between the assertion of S and the assertion that S is true (or false.), and may be stated as: Where S presupposes S' , it is incorrect for a speaker to assign a truth-value (T or F) to S unless he believes that S' .

There is, then, it appears, implicit in the relation of presupposition, a rule that enters into the phenomenon of contextual implication as a rule or convention of correct linguistic use. What manner of rule is it?

Are we not, it may well be objected here, extending the concept of correct use of language (and, hence, the concept of linguistic rule) beyond the level of ordinary language? The examples given thus far of incorrect use of language show failure to conform to the rules of a given language, English. But a rule extractable from the relation of presupposition seems to belong more with the rules of logic than with the rules of English, or allied languages. Now the rules of logic are concerned with the validity of argument in any language in which

arguments can be formulated, not with the correct use of linguistic expressions in a given language.

In dealing with this objection, I shall not attempt to elucidate the concept of rule — the task lies outside the scope of this work. It will suffice to notice that what we call “rules” may be expressed in a great variety of linguistic forms. For example, asked “What is the rule about smoking on the campus during examinations?” I might reply in any of the following ways: “Students do not (will not) smoke during examinations,” “Students should not smoke during examinations,” “No smoking during examinations,” “Smoking during examinations is forbidden.” “Statement” and “rule” are not opposed terms in ordinary discourse nor are rules always or even often formulated in the imperative mood. The differences in the way in which we express what we call “the same rule” depend, I suspect, partly on differences in the circumstances of communication.

Also, it would take me too far afield from the main path of argument to treat in detail the relation between rules of a given language and rules of logic. But the following general points about the relationship are essential to my argument. A formula like “ $(p \cdot p > q) > q$ ”, interpreted in the usual truth-functional way, may be said to express a “law” of logic, yielding an entailment rule, (“ $p \cdot p > q$ ” entails “ q ”) or, to look at the formula another way, a rule for valid inference, (the inference from “ $p \cdot p > q$ ” to “ q ” is valid.) When, in ordinary parlance, we refer to the rules of logic, we often, if not always, have such rules for valid inference in mind. (Systems of logic, however, display other sorts of rules, namely, formation-rules.) Now, formulas like “ $(p \cdot p > q) > q$ ” contain only logical constants, and the variables, p , q , may take as values sentences in *any* language that a) can be used to make truth-functional statements, and b) are so used that the resulting statements have one or other of the two values, truth and falsity, (T and F). The formula, then, has a very wide range of application, and in this range it admits of no exceptions. Indeed, the formula is called a “law”, because it has the value T for every possible combination of truth-values of p and q . This same feature of the formula is expressed by saying that the formula is analytic or by saying that it gives us a logical rule of entailment. No matter what we are talking about or what language we are talking in, the rule holds, provided the conditions for the application of the system are met.

Quite different, in some ways, from such rules of logic are entailment (and inference) rules like the following: “X is a bachelor”

entails "X is not married," "X is a man" entails "X is an animal." These rules, as formulated, are rules of entailment between statement-making sentences in the English language, and they are limited to discourse which is about certain subjects, legal and biological. Moreover, rules like these, as we have seen earlier, may have exceptions. Entailments between sentences in an ordinary language depend upon the rules of use for the constituent words of the sentences, and rules of use in an ordinary language are often not fixed or precise, for examples, the rules for "bachelor," "married." On the other hand, entailment rules for English may have an implicit, though perhaps limited, generality. Corresponding to "X is a bachelor" entails "X is married," there are entailment rules for French, German and any other language which has a word for bachelordom, that is, for which "bachelor" and "married" have exact translations. (This implicit generality is reflected in the fact that we can state the entailment rule in question without mention of expressions in a given language: "the statement that someone is a bachelor entails the statement that he is married.")

What I have just said about formulas of the system of truth-functions will hold, *mutatis mutandis*, for logical formulas which apply, not to unanalysed statements, p , q , but to statements of certain forms, for example (employing the symbols in the usual way) statements of the forms, $f(x) > g(x)$, $x \in F$, $F < G$. Here, the conditions for the application of the system go beyond those for the application of a system where the only variables are statement-variables. The abstract expressions must be interpretable, consistently, as giving the logical forms of statement-making sentences in some language. (Thus, " $x \in F$ " is customarily interpreted as " x is a member of the class F ." The concept of class membership, it might be noted, is not culture-bound — it is essential to the language of science and mathematics.) Once this condition is met, then, formulas like " $(x \in A \cdot A < B) > x \in B$ " are laws of logic. In this example of a law of logic, the customary interpretation, in terms of entailment, would be: " x is a member of the class A and the class A is included in the class B " entails " x is a member of the class B ."

The grammatical rules of a given language are different again, both from the entailment rules of logic and the entailment rules of a given language. They are not, of course, concerned with entailment or inference and hence are in no sense rules of valid or correct reasoning or inference. They have certain resemblances to the formation-rules of a logical system (rules which give the permissible combinations of

expressions in the system) for grammarians classify expressions into different types, according to function, and in so doing tell us, for example, what is and is not a correctly formed sentence in the language. But grammarians, in general, do more than this — they speak of what is often, or commonly, “appropriate” in certain linguistic contexts, and they even formulate rules to which they list standard exceptions, indicating that there are numerous unlisted ones. At the same time, the grammatical rules of a given language may have an implicit or potential generality which may be very limited or very extensive. For example, many, but far from all grammatical rules in and for English have corresponding rules in and for German and French.

Are there not, though, it may be asked, some rules (or defining statements that yield rules) of, say, English grammar to which *any* ordinary language must conform so far as it is a language? And, if this is so, should we not be careful to distinguish between what might be called the “conventions of English” and the rules for ordinary language, whether English or Karundi or some other? The sort of *prima facie* implicitly universal rule of English grammar that comes to mind here is the rule that every sentence must have a verb. Let us look at the sort of definitions and statements from which this rule is derived. I quote from a nineteenth century grammar:

A *sentence* is a collection of words of such kinds, and arranged in such a manner, as to make some complete sense. By “making some complete sense” is meant, that *something is said about something*. It is plain, therefore, that every ordinary sentence must consist of two essential parts:

1. *That which denotes what we speak about*. This is called the *Subject*.

2. *That which is said about that of which we speak*. This is called the *Predicate*.

... It is the essential function of a Substantive (Noun or Pronoun) to denote some thing about which we speak. It is the essential function of a verb to denote what is predicated respecting that of which we are speaking.²⁷

If we look at this passage critically we shall see that there are (at least) two very different possible interpretations for the rule that every sentence must have a verb. On one reading, the rule becomes the rather vague truism that, if one wants to say something about the world, one must say something about something. (I employ “truism” here in the ordinary sense of a commonplace, reserving “tautology” or “analytic formula” for the precise truths of logic.) This truism is

perhaps vague enough to apply to any or almost any natural language, and it points to requirements no less important for being very flexible. But on another reading, the rule refers to sentences, as distinct from words, and to words that are classifiable as nouns or pronouns, and as verbs, and so on. On this interpretation, the rule is both more precise and more limited in its applications. Not all languages, the linguists tell us, lend themselves to subject-predicate, noun-verb, categorizing. In addition, the rule in its more precise form has exceptions, even in English. In certain contexts "A rabbit" will be considered a sentence. Even the distinction between word and sentence in English grammar can, I think, be plausibly challenged for living English. "Read the concluding part of Joyce's *Ulysses*," some young writers of the "beat generation" have said to me, "and listen to people talking on the telephone and you'll see that the division of English into words and sentences is arbitrary and distorting." I have done both with the beat doctrine in mind and am inclined to concede that these writers are not totally wrong. English can certainly be used so as to seem to flow like a river, without segments or parts.

What we may conclude from these facts about language and languages is, I think, this. Certain (not all!) rules of a given grammar will probably reflect, point to, indicate (a vague word is needed here) some extremely broad, not precisely specifiable requirements of a natural language. These requirements can be met in a great diversity of ways by different linguistic conventions in different languages or language groups. There are consequent dangers in two directions. On the one hand, philosophers with a temperamental bent towards generalization may be led to endow a fairly precise but limited rule of their native grammar with the wide applicability which only the vague requirement has. On the other hand, linguists with a leaning towards scientific exactness may remind generalizing philosophers sharply that "there is no Language, there are only languages" and fail to see broad, but instructive similarities of function in the variety of linguistic devices.²⁸

All this has bearing on Strawson's doctrine of presupposition, that is, his account of the relation and the rule yielded by it. In the discussion that follows, I shall, for reasons of both brevity and relevance, confine myself to the case of the presupposition of statements like *The S is P*. In this case, the doctrine is limited in its application to languages that display a subject-predicate structure and that have expressions corresponding to the definite article in English. (The

doctrine is applicable, I shall argue, to *any* ordinary language only in a vague and truistic form.) I propose to show that, in the case in question, the doctrine of presupposition consists of two logically independent parts. The first part is concerned with the function of the definite article "The" (and other uniquely referring terms) and may be confirmed by reading any English grammar. But the second part, concerned with the presupposition of statements like *The S is P*, while it is a natural enough development of English grammar, is not a necessary one. It answers a question raised by English grammar which can be answered plausibly in more than one way. In brief, Strawson's doctrine is an explicatory logical model. As such, there is much to be said for it, but one cannot say for it that the ordinary man always, or for the most part, follows it, because he does not operate with logical models at all. Finally, I shall try to show, in showing to what the logical model applies, that both Strawson and his critics have confused the domain, the range of application, of a linguistic rule with an exception to the rule. This distinction is as important in connection with linguistic rules as it is in connection with ethical rules, though perhaps more strikingly apparent in the latter instance. We are all aware that if a dedicated hedonist should produce pain in others and himself by sleep-walking, his nocturnal behavior would not be an exception to the rule to maximize pleasure. The rule, we know, applies only to situations where concepts like those of choice, deliberation, intention, and so on are appropriate. For something to count as an exception to a rule (in the usual sense of "exception") it must occur within the domain of the rule. I shall, throughout the whole discussion, concentrate on those issues that concern the concept of correct use of language and are, accordingly, pertinent to the explanation of "contextually implies."

The first part of Strawson's doctrine, in the case in question, consists in an account of rules for the referring use of expressions as distinct from rules for the descriptive (ascriptive, attributive) use. I find it difficult to imagine how anyone could disagree with his three main points here. The first is that "... to refer [by using some conventional device for so doing] is not to say you are referring."²⁹ Surely, if I have several children and say "My child is asleep," I might, if the context does not make the reference clear, be asked, "To which of your children are you referring?" I should in reply then presumably *say* to which child I was referring, but I did not say anything about my act in the first remark. The second main point is "The requirement

for the correct application of an expression in its referring use to a certain thing is . . . that the thing should be in a certain relation to the speaker and to the context of utterance.”³⁰ Thus, the rule for the correct referring use of “I” mentions the relation of identity with the speaker. Now, rules for the correct descriptive (ascriptive, attributive) use of expressions do not mention relation to the speaker or the context of utterances. For example, the requirement for correctly describing something as “animal” or “vegetable” or “mineral” is simply that the thing should have the characteristics specified in the definitions of these terms.

The third main point is that there is an important difference between the two sorts of rules. One might put the difference in the following way without going beyond accepted matters of English grammar. The fulfillment of the requirement for a correct referring use of an expression is not part of what is stated by such a use *in the way in which* the fulfillment of the requirement for a correct ascriptive (descriptive, attributive) use of an expression is part of what is stated.³¹ For example, if I make the statement, *This one is black and that one is white*, the fact that the one referred to by “this” is nearer me than the one referred to by “that” is not part of the predicate, not part of what I say about the two objects. But I do say, state, or predicate of the objects that one fulfills the condition for the (correct) ascriptive use of “white” and the other the conditions for the (correct) ascriptive use of “black.” We might note here that, according to the explicatory model, a speaker, in employing a referring expression, will contextually imply that he believes that the conditions for its correct employment are fulfilled.

All three main points are, I think, made fairly explicit in English grammars. The old grammar from which I have already quoted explains the use of “the” rather nicely as follows.

Demonstrative Adjectives . . . point out that which we are speaking of by indicating some relation which it bears to ourselves, or to some other person or thing. This class includes: 1. The Definite Article the . . . The Definite Article the is used to designate among all things denoted by a noun that one, or those, that we are speaking of. . . . It does this (1) by directing attention to some previous mention of the thing; . . . (4) The also indicates that particular thing with which we have some obvious connection, . . . The definite article does for objects in the sphere of conception what the demonstrative that does for visible objects within our view.

This and *that* may be used as *real demonstratives* (to point to things themselves). In this case, *This* points to what is 'near me.' *That* points to what is 'at a distance from me,' . . .³²

Now, from this account it is surely clear that the function of expressions like "The S" is to refer or point to something that has a certain relation to the context of utterance as well as having certain characteristics. Also, it follows that if, on an occasion of such referring use, there is no S to which I am referring, the reference fails, the pointer points at nothing. (Note that the correct use of expressions like "The S" does not require that there should be in the world one and only one S, but that there should be one and only one S to which I am, in a given context, referring, for example, one and only one black horse in the field or one and only one table in the room.) Here, an unwarranted limitation of "refer" to "successfully refer," sometimes adopted by philosophers, may lead to the following mistaken formulation of the negative form of the relevant linguistic rule: "'The S' is incorrectly used if there is nothing to which it refers." This way of formulating the incorrect use of referring expressions would make Shakespeare guilty of a solecism when he put "Is this a dagger that I see before me?" into Macbeth's mouth. The formulation also goes against the usual account of what I am doing when I speak of *the daughter of Mrs. J. Smith*, either mistakenly thinking that she has one and only one daughter, or intending to deceive my audience about the progeny of Mrs. J. Smith. In the one case, I am usually said to have made a mistake of fact, not of language. In the other, I take advantage of a rule of correct use in order to deceive.

These fairly straightforward matters of grammar lead to a question for which grammarians provide no answer. The question arises in this fashion. When we engage in discourse about objects in our environment, we refer, or point, in order to say something about the referents, that is, to make statements, issue commands, evaluate, ask questions, and so on. Suppose, now, that an expression, "The S", fails to refer to anything. What shall we say about statements, commands, and so on, made by employing "The S"? Are the statements true or false, the commands, legitimate or illegitimate, the questions, pertinent or not, the appraisals, just or not?

In considering the import of this question, I shall confine myself to its application to statements. I shall also postpone for a little an interrelated question of vocabulary. If we reply that, under the conditions specified, no true or false statement is made, should we not

add that no statement at all is made, that a meaningful statement-making sentence, fails, in this instance, to be used to make a statement?

The second part of Strawson's doctrine of presupposition consists in answering in one way the main question I have raised, namely: what shall we say about a stating use of "The S is P" or "The S is not P" when "The S" fails to refer to anything? For the moment, I wish to point out, first, why grammarians cannot provide an answer to the question and, second, why there is more than one plausible way of answering it.

The question raises the interrelated issues of what is entailed by statements like *The S is P*, of the rationale of the relative assigning of truth-values to statements of various forms, and of the validity of certain types of argument. For example, if one answers the question by saying that, when "The S" fails to refer to anything, both *The S is P* and *The S is not P* are false, then these forms of statement cannot be taken as contradictories. This choice is part of Bertrand Russell's well-known Theory of Descriptions, and it follows from his analysis of statements like *The S is P* as entailing a uniquely existential statement, *There is one and only one S*. An answer to the question thus commits one to a systematic theory of certain logical transitions and this is a matter that lies outside the province of the grammarian.

English grammar, then, cannot dictate an answer to our question; nor, since English grammar is not sacrosanct, can we judge the plausibility of answers solely on the basis of their being a "natural development" of certain grammatical analyses. Russell's answer rejects the grammarians' Subject-Predicate analysis of statements like *The S is P* and puts them in the category of the $\exists (x) f(x)$ form. But there is no reason why, particularly if one is interested in *The S is P* form in the field of mathematics, one is committed to the grammarians' analysis as the best clue to the logical force of statements. On the other hand, if one is interested in everyday reasoning in ordinary English, the grammarians' account of the function of types of terms and analyses of types of sentence are pertinent to one's inquiry.

Let us look now at the second part of Strawson's doctrine which answers in one way the question under discussion. Statements of the form *The S is P* (or *is not P*) do not entail, but presuppose uniquely existential statements of the form *There is one and only one S* (that is being referred to). According to Strawson's definition of presupposition, then, when "The S" fails to refer to anything, statements like *The S is P*, made with the use of "The S," are not true and are also not

false. The rule for the correct use of language in this instance of presupposition is: it is incorrect for a speaker to assert or to deny (or to assign T or F to) statements of the form *The S is P* unless he believes that there is one and only one S that is being referred to (or believes that a statement of the form *There is one and only one S . . . is true*).

It lies far beyond the scope of this work to consider the relative merits of Strawson's answer compared to the answer of Russell to the question raised. However, it is important for my purposes to point out that neither Strawson's nor Russell's answer can be proved right or wrong by a check on what the ordinary man does, linguistically. For the ordinary man is concerned either with getting his "message" through to his audience in not very garbled form (he will stand some garbling as unimportant) or with seriously misleading, deceiving, those at the receiving end. Either purpose can be accomplished without adopting a consistent system of employing linguistic expressions. Moreover, the fact that the ordinary man makes no precise distinction between entailing and presupposing and uses "false" broadly to characterize a variety of linguistic mishaps will make a good deal of his speech behavior appear to support Russell's analysis. But each analysis of *The S is P* is properly judged, not as an empirical generalization about how people apply "true" and "false" to such statements, but as an explicatory model which is part of a larger systematic explanation for a number of linguistic and logical phenomena. Thus, Strawson's doctrine of presupposition should be judged for its power to illuminate the relation between certain rules of English grammar and the following phenomena: we often (not always) treat the singular forms, *The S is P* and *The S is not P* as contradictories and take *The S is not P* as equivalent to *The S is P is false* (these relations do not hold for Russell's analysis); we often (not always) refuse to accord a truth-value to *The S is P (or is not P)* when "The S" applies to nothing. Looked at in this way, Strawson's doctrine has much to be said for it. It explicates well certain lines of reasoning, certain equivalences, in ordinary discourse (in English and allied languages) and it is a natural and plausible development of the grammar of referring terms (in English and allied languages).

It should now be clear why the formulation of the relation of presupposition in language that suggests that it is a truth-functional relation is unfortunate. The *p*'s and *q*'s of the truth-functional system have, by stipulation, the value T or the value F. Nothing, that is to say, counts as a statement for the truth-functional interpretation of the

system unless it is either true or false. So, when S' , a presupposed statement, is false, the case of S , the presupposing statement, falls outside the formulas of the system. For, in this case, S doesn't have any truth-value. The phenomenon is quite unlike the fact that the formula " S or non- S " does not hold in a 3-valued system when S has the third truth-value. No matter what number of truth-values we are operating with, S in the case at issue has none of them.

An interesting question arises here. Should we, in this peculiar case where S has no truth-value, continue to call S a "statement"? Strawson apparently replies "yes" in some contexts and "no" in others, and there is an air of paradox about either an affirmative or a negative answer.³³ The reason for this is perhaps that, for obvious reasons, when we think of a statement as an act of stating, we are inclined to an affirmative answer, but when we think of a statement as what is stated (in abstraction from the act) we are inclined to a negative answer.

This difficulty in nomenclature points to the conclusions which may be drawn from our discussion of the definition of presupposition and of the consequent rules for correct linguistic use. The definition and rules concern a condition that must be met before a statement can play a role in reasoning, can function as an element in logical argument. (A rough paraphrase of the definition of " S presupposes S' " might run "the truth of S' is a necessary condition for treating S as an element in reasoning.") Accordingly, the general rule for the use of a statement S that presupposes a statement S' , falls outside the rules of systems of logic and is properly called a "rule for the correct use of language."

We can, perhaps, say truly (and very vaguely) that all ordinary languages will exhibit some presuppositions for the operation of argument or reasoning and also that all ordinary languages (of a certain minimum degree of complexity) will have *some* rules of reference as well as rules of entailment. But it seems highly dubious that the rule about the assigning of truth-values to statements like *The king of France is bald* should have corresponding versions in all ordinary languages or even that the relation of presupposition between statements should have corresponding versions in all ordinary languages. The systems of formal Logic achieve their great generality and freedom from exceptions to rules precisely because of their abstractions from ordinary discourse. Rules which are developed from English grammar, like the rule about the use of "The S ," give us more insight into English (and allied languages) than systems of Formal Logic

ever can. We should be very careful, however, not to immediately take such rules as delineating a structure common to all languages, something that might be appropriately called "the Structure of Ordinary Language." Whether or not there is something like this (and, of course, there *may* be) can only be determined after an extensive investigation of many very different groups of ordinary languages.

We are now in a position to consider the matter of exceptions to the rule derivable from the relation of presupposition. Let us remind ourselves of the content of the rule: when S presupposes S', it is incorrect for a speaker to assert or deny S (or assign T or F to S) unless he believes S' (or assigns T to S'). Unfortunately, both Strawson and his critics have treated the case of the man who tells me "The lodger next door has offered me twice that sum" when he knows that there is no lodger, as an exception to the rule. We do, of course, if we discover the man's attempt to deceive, brand him a "liar" and call his statement "false." We do not say, "Well, you *are* trying to deceive me, but you do not lie, because what you state is, according to a rule of ordinary language, neither true nor false, and, to be a liar, one must deliberately make a false statement." But is this case of assertion of S by the speaker and subsequent assigning of F to S by the hearer, an exception in the usual sense?³¹ We have already seen that rules of grammar, like the rule giving the use of "the S" for example, are not interpreted so as to make a would-be deceiver's employment of the expression incorrect. Another way of putting the matter is to say that rules of this sort apply to, are formulated for, normal acts of stating (or normal stating contexts) only. The case of the speaker intending to deceive lies outside the domain of the application of "correct" and "incorrect" in the rules. Now, could we have expected or reasonably demanded to get anything else? The deception intended in the example in question *could not take place* unless there were a generally recognised rule observed in the normal act. It is just because a statement of the form *The S is P* is not, when the assertion is normal, asserted (or denied) unless a statement of the form *There is one and only one S* is believed by the speaker to be true, that the liar can deceive me. He trades on our presumptions of normality. Moreover, ordinary language, which can adapt itself marvellously to differences of practical importance, is surely not inconsistent in giving us, the hearers of the would-be deceiver, a special rule for assigning truth-values in this abnormal context. When a man tries to deceive us, we need a way of giving him the lie.

I shall omit, as unnecessary here, consideration of how we assign truth-values to a statement of the form *The S is P* when there is no S, and the speaker is not trying to deceive, but has made some linguistic mistake in formulating S. However, it is of interest that Strawson takes this other case of a non-normal act of stating as an exception also to his rule. (His example is the saying, in a stating context of "The United States Chamber of Deputies contains representatives of two major parties." We might, he says, call the statement "true."³⁵ And the third kind of "exception" he admits is the case in which a uniquely referring expression without a referent, occurs in the grammatical predicate, not as the grammatical subject. Clearly, a more precise formulation of the rule would put this case also outside its domain.) The results of confusing domain of application and exception is that both Strawson and his critics debate the issue as though his doctrine were an empirical generalization, to be shown true or false, reliable or unreliable, according to the number of exceptions which can be produced. Meanwhile, the exceptions, in the usual sense, which there are to the rule, go unnoticed. What are these exceptions?

We have seen that Strawson's whole doctrine is to be taken as an explicatory logical model, not as an empirical generalization and that the rule about assigning truth-values applies only to normal acts of stating. Now, even within this domain, it is very unlikely that Strawson's rule would be observed in all situations and all circumstances of communication. Our everyday talk varies in a myriad of ways according to differences in the situation talked about and in the circumstances of communication. The logician of ordinary language, so far as he is worthy of the title "logician", does and must abstract from the countless variations in our way of using linguistic expressions. In this respect, he is not different from the logician interested in the logic of the precise sciences, who does and must abstract from variations in the linguistic behavior of physicists and mathematicians.

Strawson's doctrine explains well and his rule is observed in what might appropriately be called "standard cases" within normal stating contexts. The standard case for which rules of logic are formulated is determined, I think, partly, but not wholly by considerations of frequency. It is the sort of case that happens often enough to make the formulation of a rule worthwhile. (For example, in the span of our language-using life, we pretty often have occasion to ask whether or not there is a So-and-So. And, until the question is answered, we refrain from making statements of the sort *The So-and-So is P* and

The So-and-So is not P.) But also, the standard case is one that is structured (in various respects) simply and clearly enough to call forth fairly uniform linguistic behavior. One of the ways in which the standard case should be simply and clearly structured is, of course, in the circumstances of communication. We shall rightly expect Strawson's rule to be observed when not following it would seriously garble "the message" or seriously mislead the audience. For example, if someone should say to a childless couple, "How strange that you both have black hair and your child has red hair," they surely would not (except for a joke) reply "Our child does not have red hair" or even "It is false that our child has red hair." But they might well reply, "False — we don't have any children." Here, a truth-value is assigned in a way that constitutes an exception to the rule, but information is given to prevent misleading the audience.

The following exception, which I have encountered, is a good illustration of a non-standard case. In the United States, it happens fairly often that one's phone rings and a voice says something like "This is the . . . Survey Service. Are you listening to your T.V.?" (The first troublesome variation in this sort of case is found in the form of question. Sometimes it is "Are you listening to a T.V.?" Sometimes "the" is used instead of "a"; sometimes, "Is anyone in your house listening to T.V.?") I have made no statistically respectable investigations of answers to the question when the answerer has no T.V. set. I have, however, polled five intelligent, well-educated people who are interested in speaking precisely and who have no T.V. set. The results were as follows. Only one person, who had studied philosophy, thought it misleading to answer "No". The others, who had not studied philosophy, thought a "No" answer was not misleading "because it's true. If I have no T.V. set, then I cannot be listening to it." (This case might seem to fall outside the domain of the rule as one in which the uniquely referring expression which has no referent occurs in the grammatical predicate, not as the grammatical subject. But the four persons were just as inclined to say "No," to the question formulated as "Is your T.V. being watched by anyone (or by you)?") One of the four sometimes answered "No," sometimes "I have no T.V." depending on how busy he was when the phone rang. The only point of agreement among the five was that the answer "I have no T.V." gave more detailed information than "No". Also, I might add, no one of the five ever answered "Yes."

The case in question is further complicated by the fact that the

questioner does not indicate clearly whether the Survey Service is interested in finding out whether the questionees have a T.V. set — are there three columns for answers? (I suspected, but did not settle the matter, that the negative answer seemed right to the questionees because they believed that the main interest of the questioner was in whether programs were or were not being seen at a certain hour, and not in the reasons why they were or were not being seen.) It is also complicated by emotional factors. Most of us are annoyed at being disturbed by calls from Survey Agencies, many of us do not like to give out any information about ourselves to strangers, and so on. Accordingly, the case is not a good standard one for the formation of logical rules. Rules, whether of ethics or logic, are useful when formulated for standard cases, of conduct or of discourse.

What I have said about standard cases applies, of course, to the explicatory model. I cheerfully admit that the following set of answers to questions might not be logically odd or absurd in all cases and in all circumstances of communication. “Did you make the statement p ?” “Yes” “Were you using language correctly?” “Yes” “Were you lying?” “No” “Did you believe that p ?” “No” or “I don’t know.” We might, for example, be inclined to give the latter answer to the last question, when we weren’t quite sure whether we believed something and our questioner was particularly interested in the degree of our conviction. The dichotomy, either one believes p or one does not believe p , is not always serviceable in ordinary discourse.

We have now seen how, according to the explicatory model for contextual implications, “presupposes” and “contextually implies” are related, and how correct use of language enters into what a speaker contextually implies by employing expressions like “The S is P ,” “The S is not P ” in a stating context. Our exploration also revealed something of the variety of linguistic phenomena covered by the concept of correct use of language.

There remains one more question to tackle. I have postponed to the conclusion of my study, the question of the nature of logical oddity. Logical oddity is, as it were, the negation of a contextual implication. Just what is it to “negate” a contextual implication and how does a logically odd act of statement differ from a contradiction?

Let us begin with contradiction. Consider the two sentences, “Mr. Jones has gone out” and “Mr. Jones has not gone out.” It is plain, as Strawson has pointed out, that the two *sentences* are not contradictory, for they might be said at different times and places and the “Mr.

Jones" might have different referents. But the sentence "Mr. Jones has gone out and Mr. Jones has not gone out" can appropriately be called a "paradoxical" sentence, for it is one which, on a standard interpretation of its employment, could only be used to make a self-contradictory statement. The standard interpretation of "and" (conjoint assertion) rules out difference in time and place of asserting the two conjuncts as well as difference of speaker, and the standard interpretation of the double occurrence of "Mr. Jones" rules out ambiguity of reference. Also, "gone out" on a standard interpretation, is taken as having the same sense in each conjunct. (One can, as we noticed earlier, easily imagine non-standard ways of taking the sentence on an occasion of its employment which would make the resulting statement self-consistent.)

Now, granted the standard interpretation of the sentence, any use of it (in a stating context) involves two things: First, a performance, a linguistic act, that is pointless, or absurd. For the speaker, in part of his linguistic act undoes what he does in the other. He at once states and unstates, or states and denies. Accordingly, his act is self-defeating. Second, considering "statement" in its meaning, not of the *act* of stating, but of *what is stated*, then, nothing is, in the usual sense of the word, stated. Logic rightly abstracts from speakers and acts of stating, and treats only of what is stated, what is true or false.³⁶ In the interests of systematic treatment of these abstractions from the act, logic again, quite rightly, treats self-contradictions as limiting cases of statement. A self-contradiction ($p. - p$) then, is a statement which under no state of affairs can be true, or which must be false. It is the denial or negation of that other limiting case of statement ($p > p$) which cannot be false, or which must be true.

There is nothing in the phenomenon of contextual implication which corresponds to the oddity or paradox of a self-contradictory statement. For, if we abstract what is stated from the act of statement in the paradigm of contextual implication, the result is a self-consistent statement. Thus, if Mr. Smith says (in a stating context) "Jones has gone out, but I don't believe it," two statements (in the logician's sense) have been made, namely: *Jones has gone out* and *Smith doesn't believe that Jones has gone out*.³⁷ These statements are obviously consistent. It is, I think, the absence of formal contradiction in the paradigm that makes it natural for us to attribute contextual implication to the act.

If now we turn to the act, and the sentence employed in the act, some resemblance to the *making* of a self-contradictory statement

appears. The sentence "He has gone out, but I don't believe it" is, on a standard interpretation and where a presumption of normality is made, a logically odd sentence. The act of saying the sentence, in a certain context, would be, in our culture, an act which entitled the hearer to assume that the speaker believed what he said. If the speaker then cancels the implication, lets the hearer know that the assumption is unjustified, his performance is self-defeating, pointless, or absurd. The absurdity in this instance, however, is culture-bound, while the absurdity of stating a contradiction is not. Suppose a Murundi in court, in the situation in which, as we have seen, lying is taken for granted says, "He went out" and then adds, "I don't believe it." The remark might, perhaps, be taken as pointless, but the pointlessness might be that of redundancy, rather than of undoing what one has done. In contrast, to say "He has left and he has not left" (assuming absence of ambiguity) is, in any culture or language, an absurd or pointless linguistic act.

SUMMARY AND SURVEY

In this essay, I have rejected the inductive interpretation of the paradigm of contextual implication (to say "p" is to imply that one believes that *p*) and proposed in its stead an explicatory model according to which a speaker in making a statement contextually implies whatever one is entitled to infer on the basis of the presumption that his act of stating is normal. In developing this model, I showed how contextual implication depends in the following ways on three distinct matters. 1. *A stating context*. For the implication to hold, a speaker must say something in a stating context. To say "p" in a non-stating context (play-acting, for example) is not to imply that one believes that *p*. 2. *Presumptions of normality*. Within a stating context, the implication holds only if certain presumptions of normality in the act of stating are made. These presumptions are not inductively established — they function as principles of communication. An act of stating is evaluated as "normal" (ordinary language use of "normal") only if the speaker believes what he says. When a man believes what he says, the following two conditions must be fulfilled: Intention to tell the truth (lack of intention to deceive) and correct use of language. These two conditions, then, form the content (or part of it) of the presumption of normality. 3. *Rules for the correct use of an expression*. Given a stating context and presumptions of normality, then *whether* belief is implied when a

speaker says "p," and, if so, *what* belief or beliefs, will be determined by rules for the correct employment of "p" and its constituents. For example, if "p" is a command, belief is not implied; and if "p" is of the form *The S is P*, then what beliefs are implied is determined by rules for the correct use of expressions like "The S."

In discussing the presumptions of normality and rules for the correct use of expressions, I have tried not to confuse what is culture-and-language bound with what is culture-and-language free.

In conclusion, I should like to point out that there seem to be other kinds of contextual implication (less basic than the paradigm) requiring presumptions in addition to that of the normality of the act of stating. For example, it has been said that "... whenever we make a statement in a standard context, there is an implied claim to its reasonableness, ..."³⁸ [That is, the speaker implies that he believes that there is some evidence for it]. The presumption of a certain kind of rationality is required to explain this implication. Moreover, "'ought' implies 'can'" appears to call for the presumption of still another sort of rationality of the speaker. Finally, if, in saying "I know it is so," I imply that I have confidence that it is so, then still another kind of rationality seems presumed.³⁹ But presumptions of rationality are another story, though similar to the one that I have just completed.

FOOTNOTES

¹ P. F. Strawson, "On Referring." Reprinted in *Essays in Conceptual Analysis*, ed., Antony Flew (Toronto, The Macmillan Company, New York, St. Martin's Press Inc., 1956), p. 54.

² J. L. Austin, "Other Minds." Reprinted in *Logic and Language*, ed., Antony Flew (Oxford, Basil Blackwell, 1953), pp. 142-3.

³ See P. H. Nowell-Smith, *Ethics* (Melbourne, London, Baltimore, Penguin Books, 1954), pp. 82-83.

⁴ J. O. Urmson, "Parenthetical Verbs." Reprinted in *Essays in Conceptual Analysis*, (See footnote 1), p. 196.

⁵ Paul Edwards, *The Logic of Moral Discourse* (Glencoe, Illinois, The Free Press, 1953), pp. 21 ff.

⁶ See G. E. Moore, "Russell's Theory of Descriptions," *The Philosophy of Bertrand Russell*, The Library of Living Philosophers, ed., Paul Arthur Schilpp (Evanston and Chicago, Northwestern University Press, 1944), p. 204.

⁷ Paul Edwards, op. cit., p. 29.

⁸ P. H. Nowell-Smith, op. cit., pp. 80-81.

⁹ P. H. Nowell-Smith, *ibid.*, p. 81.

¹⁰ I here and elsewhere abbreviate "statement-making context" to "stating context".

¹¹ P. H. Nowell-Smith, *Ibid.*, p. 179.

- ¹² C. K. Grant, "Pragmatic Implication," *Philosophy*, Vol. XXXIII (October, 1958), pp. 303-24.
- ¹³ C. Lewy, "Some Notes on Assertion." Reprinted in *Philosophy and Analysis*, ed., Margaret MacDonald (New York, Philosophical Library, 1954, pp. 120-24.
- ¹⁴ P. F. Strawson, "A Reply to Mr. Sellars," *Philosophical Review*, Vol. LXIII (April, 1954), p. 224.
- ¹⁵ See J. L. Austin, "A Plea for Excuses," *Aristotelian Society Papers*, 1956-7, p. 6.
- ¹⁶ Ibid., p. 7.
- ¹⁷ See Benson Mates, "Comments on the Foregoing," *Inquiry*, Vol. I (Winter, 1958), p. 240.
- ¹⁸ Strawson's two remarks about contextual implication could be formalized as follows without the paradoxical consequence noted earlier.
 1. "x in a normal stating context says 'p'" entails "x believes that p".
 2. "x in a stating context says 'p' and does not believe that p" entails "Either x is using language incorrectly or x is intending to deceive."The first entailment could be taken as making explicit a criterion for the evaluative use of "normal" applied to stating contexts; the second could be taken as making plain two conditions of abnormality in such contexts.
- ¹⁹ The general formula for contextual implication — "by saying 'p', a man implies he believes that p" — does not hold when "p" is not a statement-making sentence, for example, when "p" is "close the door."
- ²⁰ There are, of course, situations like play-acting, where a distinction between situation and manner seems otiose.
- ²¹ J. L. Austin, op. cit., pp. 15, 16.
- ²² Ethel Albert, from a talk delivered in the Belgian Congo, to be published in *Cahiers des études africaines, École Pratique des Hautes Études*, Paris (December, 1959).
- ²³ Ethel Albert, from Chapter XIV (a), *Logic and Rhetoric*, of a work in progress, *Values, Beliefs and Conduct: A Study of Urundi*.
- ²⁴ Ibid.
- ²⁵ P. F. Strawson, op. cit., p. 216.
- ²⁶ Ibid., loc. cit.
- ²⁷ C. P. Mason, *English Grammar*. (London, Bell and Sons, 1887), pp. 141-2.
- ²⁸ See P. F. Strawson, *Introduction to Logical Theory* (London, Methuen and Co., New York, John Wiley and Sons, 1952), pp. 225-6. I have slanted my conclusion somewhat differently.
- ²⁹ P. F. Strawson, "On Referring," op. cit., p. 37.
- ³⁰ Ibid., p. 42.
- ³¹ Ibid., p. 43.
- ³² C. P. Mason, op. cit., pp. 40, 47, 53.
- ³³ In "A Reply to Mr. Sellars." Strawson writes throughout of "statements" that "lack a truth-value". However, in *Introduction to Logical Theory*, he writes (p. 185) "For a sentence of the statement-making type to have meaning, it is not necessary that every use of it, at any time, at any place, should result in a true or false statement. It is enough that it should be possible to describe or imagine circumstances in which its use would result in a true or false statement."
- ³⁴ I have already noted the difference, which may be important in some contexts, between asserting that S and asserting that S is true (or false). However, Strawson apparently takes the rule in question to concern both cases, equally.

³⁵ P. F. Strawson, "A reply to Mr. Sellars," p. 227.

³⁶ It was necessary for the development of systems of symbolic logic that logicians abstract from all conditions of utterance and action. One consequence of this needed abstraction is that the symbolic logician is concerned only with the sort of sentence that need not be distinguished from statement, namely, sentences such that, if their use by one speaker at one time, etc., results in a true statement, their use by any other at any other time, etc. will result in a true statement.

³⁷ From the viewpoint both of the logician and the lawyer, the sentence "I was born in California" spoken by me, makes the same statement as the sentence, "Mrs. Hungerland was born in California." However, there are differences between the two sentences which the philosophers of language have not yet, I think, dealt with satisfactorily.

³⁸ J. O. Urmson, "Parenthetical Verbs," *op. cit.*, p. 201.

³⁹ "I know..." in the relevant sense entails "I have overwhelming evidence..." but does not entail "I am confident...". A neurotic person may be aware that he has overwhelming evidence that his spouse is faithful or that he does not have cancer, but be unable to say truly, "I am confident."

LOGICAL NECESSITY, PHYSICAL NECESSITY, ETHICS, AND QUANTIFIERS

by

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Some philosophers, for example Quine, doubt the possibility of jointly using modalities and quantification. Simple model-theoretic considerations, however, lead to a reconciliation of quantifiers with such modal concepts as logical, physical, and ethical necessity, and suggest a general class of modalities of which these are instances. A simple axiom system, analogous to the Lewis systems S1—S5, is considered in connection with this class of modalities. The system proves to be complete, and its class of theorems decidable.

1. Introduction. This paper¹ is intended to contribute to the problem of interpreting 'it is logically necessary that', 'it is physically necessary that', and 'it is obligatory that'. The interpretations given below were suggested by certain logical analogies between these phrases and the universal quantifier. The interpretations are to be given in an extensional metalanguage; in particular, the metalanguage is not to contain any of the phrases themselves. Further, the interpretation is to be such as to permit the use of the phrases in conjunction with quantifiers.

It is only in satisfying this last requirement that my interpretations can be called original. They are based on the following unoriginal considerations. Let Φ be a sentence. Then 'it is logically necessary that Φ ' is true if and only if Φ is a theorem of logic; 'it is physically necessary that Φ ' is true if and only if Φ is deducible from a certain class of physical laws which is specified in advance; 'it is obligatory that Φ ' is true if and only if Φ is deducible from a certain class of ethical laws which is again specified in advance.²

In these cases, Φ was assumed to be a sentence. The interesting cases, however, are those in which the formula Φ is *not* a sentence; that is, when Φ contains free variables, as in 'for some x , it is logically necessary that x is identical with the Evening Star.' This example is advanced by Quine (in 'Notes on Meaning and Necessity') to support grave doubts concerning the joint use of modalities and quantification. The

example seems to follow from the true sentence, 'it is logically necessary that the Evening Star is identical with the Evening Star'; yet the example itself seems to be either false or meaningless. What is that x which is necessarily identical with the Evening Star? Is it the Evening Star, that is, the Morning Star? It seems not, since it is only an empirical fact, not a matter of logical necessity, that the Morning Star is identical with the Evening Star.

For the phrase 'it is logically necessary that' Carnap (in the *Journal of Symbolic Logic* vol. 11 (1946)) has already given an interpretation which permits the introduction of quantifiers. I shall shortly indicate relations between his interpretation and mine.

In section 3 it is observed that my interpretation of the four notions mentioned in the title fall under a certain comprehensive scheme of interpretation, and further that the valid sentences of the scheme can be axiomatically characterized by a certain system of modal logic which has hitherto not been considered in the literature.

2. *An elementary modal language; interpretations.* I wish to consider a language S which contains *individual variables*; an unspecified number of *individual constants*; for each $n \geq 0$, an unspecified number of *n-place predicates*; all the *sentential connectives*; and the additional symbol 'N'. Thus an *atomic formula* of S is the result of writing an *n-place predicate* followed by n *individual signs* (that is, variables or individual constants). All atomic formulas are *formulas*; any truth-functional compound of *formulas* is again a *formula*; if Φ is a *formula*, then so is ' $N\Phi$ '; nothing is a *formula* except as required by the foregoing rules. In particular, the formulas of S contain no quantifiers.

Now if 'N' is read 'it is logically necessary that', our intuition convinces us of the following facts:

- 1a) If Φ is tautologous (that is, passes the truth-table test), then Φ holds.
- 1b) If Φ is tautologous, then ' $N\Phi$ ' holds.
- 2a) ' $N(\Phi \supset \Psi) \supset (N\Phi \supset N\Psi)$ ' holds (for any formulas Φ and Ψ).
- 2b) ' $N[N(\Phi \supset \Psi) \supset (N\Phi \supset N\Psi)]$ ' holds.
- 3a) ' $N\Phi \equiv NN\Phi$ ' holds.
- 3b) ' $N(N\Phi \equiv NN\Phi)$ ' holds.
- 4a) ' $\sim N\Phi \equiv N \sim N\Phi$ ' holds.
- 4b) ' $N(\sim N\Phi \equiv N \sim N\Phi)$ ' holds.

All the formulas mentioned can be proved in the Lewis system S5 (one of the classical systems of modal logic proposed by C. I. Lewis;

see Lewis and Langford, *Symbolic Logic*).³ The intuition may well balk at 3a—4b; in fact, iterated modalities play little part in ordinary discourse, and the laws governing them are ill-determined. But the logic of modalities is considerably simplified by the principles 3a—4b. It is hoped, therefore, that the scrupulous reader will grant them provisionally.

But now let us read 'N' as 'it is *physically* necessary that'. We find that all the principles 1a—4b continue to hold (or at least do not clearly fail). Let us read 'N' as 'it is *obligatory* that' (or 'it *ought to be the case* that'). Again 1a—4b do not seem totally implausible. Also, surprisingly, we find that 1a—4b hold when 'N' is read 'for all x' (the universal quantifier on the variable 'x'). Furthermore, it is possible to introduce by definition an operator dual to 'N'; in fact, ' $\Diamond\Phi$ ' is put for ' $\sim N \sim \Phi$ '. For each of the readings I have given for 'N', there is a corresponding natural reading for ' \Diamond '. Thus ' \Diamond ' may be read 'it is logically possible that', 'it is physically possible that', 'it is permissible that', or 'for some x'. (Von Wright, in *An Essay in Modal Logic*, has pointed out similar analogies.)

What is the source of this strange analogy between apparently unrelated phrases? Can the analogy be exploited in order to yield interpretations for some of the phrases? In seeking interpretations let us begin in accordance with the method of Tarski ('Der Wahrheitsbegriff in den formalisierten Sprachen', *Studia Philosophica* vol. 1 (1936)). Let us consider a possible formula of S:

$$(Wxa \vee NHx)$$

(where 'W' and 'H' are predicates, 'x' is a variable, and 'a' is an individual constant). In order to make this formula either true or false we must

(1) specify a *domain of discourse*, over which the variables are to range,
 (2) assign to each descriptive constant (that is, predicate or individual constant) an *extension*, that is, assign to each individual constant an element of the domain of discourse as designatum and assign to each n-place predicate a class of n-tuples of elements of the domain of discourse, and

(3) assign *values* to the variables, that is, assign to each variable an element (to which it is supposed to refer) of the domain of discourse.

Thus we are led to *models* of our language S. These are to be ordered triples $\langle D, R, f \rangle$. D is to be a domain of discourse (that is, a non-empty set); R is to be an *extension-assignment* with respect to D, that is, a func-

tion which assigns to each descriptive constant an extension (with respect to D); and f is to be a *value-assignment* with respect to D , that is, a function which assigns to each variable an element of D .

An *interpretation* of the language S will be a definition of the phrase 'the model M satisfies the formula Φ '. Such a definition, to be interesting, must fulfill certain conditions. In the case of formulas not involving 'N' these conditions have been clearly specified by Tarski. When 'N' is involved, however, we must rely, at least for the sake of this paper, on intuition in judging the worth of a definition.

It is convenient to define first the notion of the *value* of an *individual sign* for a given model:

$$v_{\langle D, R, f \rangle}(\zeta) = \begin{cases} R(\zeta) & \text{if } \zeta \text{ is an individual constant} \\ f(\zeta) & \text{if } \zeta \text{ is a variable} \end{cases}$$

Let us now, following Tarski, attempt to construct a recursive definition of satisfaction. Certain clauses can be borrowed without alteration from Tarski:

(1) If π is an n -place predicate and ζ_1, \dots, ζ_n are individual signs, then $\langle D, R, f \rangle$ satisfies ' $\pi\zeta_1 \dots \zeta_n$ ' if and only if the ordered n -tuple $\langle v_{\langle D, R, f \rangle}(\zeta_1), \dots, v_{\langle D, R, f \rangle}(\zeta_n) \rangle$ is an element of $R(\pi)$.

(2) $\langle D, R, f \rangle$ satisfies ' $\sim \Phi$ ' if and only if $\langle D, R, f \rangle$ does not satisfy Φ ; $\langle D, R, f \rangle$ satisfies ' $(\Phi \vee \Psi)$ ' if and only if $\langle D, R, f \rangle$ satisfies Φ or $\langle D, R, f \rangle$ satisfies Ψ ; and similarly for the other sentential connectives.

It is now our desire to add a clause ' $\langle D, R, f \rangle$ satisfies ' $N\Phi$ ' if and only if . . . ' for each of the four intended readings of 'N'. We shall thus obtain four notions of satisfaction. Corresponding to the reading 'it is logically necessary that', we shall have 'satisfies_L'; corresponding to 'it is physically necessary that', 'satisfies_p'; corresponding to 'it is obligatory that', 'satisfies_E' ('E' for 'ethics'); and corresponding to 'for all x ', 'satisfies_Q' ('Q' for quantification'). For the last reading Tarski has already supplied us with an interpretation.

Let Q be the following relation between models: $\langle D, R, f \rangle Q \langle D', R', f' \rangle$ if and only if $D = D'$, $R = R'$, and $f(a) = f'(a)$ for every variable a different from ' x '. Then

(3_Q) $\langle D, R, f \rangle$ satisfies_Q ' $N\Phi$ ' if and only if, for every model M such that $\langle D, R, f \rangle Q M$, M satisfies_Q Φ .⁴

Perhaps now, instead of the relation Q which was introduced for the sake of the universal quantifier, we can define another relation which will perform the same office for logical necessity. In fact, it seems reasonable to consider 'it is logically necessary that Φ ' as

asserting that Φ holds under every assignment of extensions to its descriptive constants. Accordingly, let us define the relation L as follows:

$\langle D, R, f \rangle L \langle D', R', f' \rangle$ if and only if $D = D'$ and $f = f'$.

Then 'satisfies_L' can be defined:

(3_L) $\langle D, R, f \rangle$ satisfies_L ' $N\Phi$ ' if and only if, for every model M such that $\langle D, R, f \rangle L M$, M satisfies_L Φ .⁴

Using similar methods we can give an interpretation for physical necessity. We first fix a class K of sentences which may contain quantifiers but may not contain 'N'; these are thought of as *physical laws*. 'It is physically necessary that Φ ' is then understood as asserting that Φ holds under every assignment of extensions under which all the physical laws hold. The relation P is defined accordingly:

$\langle D, R, f \rangle P \langle D', R', f' \rangle$ if and only if $D = D'$, $f = f'$, and $\langle D', R', f' \rangle$ satisfies (in Tarski's sense) all sentences of K;

and, in terms of P, 'satisfies_p':

(3_p) $\langle D, R, f \rangle$ satisfies_p ' $N\Phi$ ' if and only if, for every model M such that $\langle D, R, f \rangle P M$, M satisfies_p Φ .⁴

In order to assure that the modal laws 1a through 4b be satisfied by all models, it is necessary to assume that for each domain of discourse D and each value assignment f there is an R such that $\langle D, R, f \rangle$ satisfies all the physical laws in K.

For the sake of the ethical notion 'it is obligatory that' we should first fix a class I of *ideal models*; these are the models in which the descriptive signs have just the extensions which they ought to have. The class I could, for example, be defined as the class of models which, in Tarski's sense, satisfy the ten commandments (formulated as declarative, rather than imperative, sentences; the fourth commandment, for instance, becomes '(x) (y) (z) (x is a person. (y is father of x v y is mother of x) \supset x honors y)'). It is important that, for any D and f, there be an R such that $\langle D, R, f \rangle$ is an element of I. The relation E is defined:

$\langle D, R, f \rangle E \langle D', R', f' \rangle$ if and only if $D = D'$, $f = f'$, and

$\langle D', R', f' \rangle$ is an element of I;

and, in terms of E, 'satisfies_E':

(3_E) $\langle D, R, f \rangle$ satisfies_E ' $N\Phi$ ' if and only if, for every model M such that $\langle D, R, f \rangle E M$, M satisfies_E Φ .⁴

3. *Generalizations.* It is seen that our four definitions of satisfaction display a common formal structure. Is this, perhaps, the reason why the four notions all obey the laws 1a—4b? The answer seems affirma-

tive, in view of the following consideration. Let X be any relation between models, let *satisfaction* _{X} be defined in precise analogy with the definitions (3_Q) , (3_L) , (3_P) , and (3_E) , and let a formula be called *valid with respect to* X if it is satisfied _{X} by every model. Then if Φ is any formula comprehended under 1a–4b, Φ is valid with respect to every relation X which fulfills the following conditions:

- (i) for all M , there is an N such that $M X N$,
- (ii) for all M, N, P , if $M X N$ and $N X P$, then $M X P$, and
- (iii) for all M, N, P , if $M X N$ and $M X P$, then $N X P$.

Another question naturally arises. Let us consider the deductive system which has 1a–4b as its axioms and *modus ponens* as its only inference rule. On the other hand, let us call a formula Φ *valid* if, for every relation X which fulfills conditions (i)–(iii), Φ is valid with respect to X . It is clear from what was just said that every theorem of the deductive system is valid. But does the converse hold? Is the deductive system complete? The answer, it turns out, is affirmative: a formula Φ is valid if and only if it is a theorem.⁵ Furthermore, there is a *decision method* for the class of valid formulas; it can be obtained by modifying suitably one of the well-known decision methods for the monadic predicate calculus.⁶

The language S may now be enlarged by the addition of infinitely many universal quantifiers, one for each variable. How is the enlarged language to be interpreted? The enlargement may be regarded as the accession to S of new modal operators. For each variable α , a relation Q_α may be defined in analogy with the relation Q (which now turns out to be Q_x). In defining satisfaction, one retains clauses (1) and (2), as well as one of the clauses (3_L) , (3_P) , or (3_E) , according as one understands 'N' logically, physically, or ethically. One adds the following clause:

$(3'_Q)$ $\langle D, R, f \rangle$ satisfies $\ulcorner (\alpha)\Phi \urcorner$ if and only if, for every model M such that $\langle D, R, f \rangle Q_\alpha M$, M satisfies Φ . ($(3'_Q)$ amounts to the same thing as Tarski's interpretation of quantifiers.)

Thus there is no difficulty in interpreting a system which contains both modal operators and quantifiers, and Quine's uneasiness in this regard seems to be without justification (provided, of course, that the interpretations I have given are 'good', whatever this may mean). The dubious cases which Quine mentions will shortly be examined in the light of my interpretation.

It is perhaps interesting to note that not only may quantifiers be

introduced into the language S, but additional modal symbols as well. For instance, one may introduce the symbols 'PN' and 'O' for 'it is physically necessary that' and 'it is obligatory that' respectively, retaining 'N' in the sense 'it is necessary that'. To obtain an interpretation, one simply combines clauses (1), (2), (3_L), and (3'_Q) with the result of replacing 'N' by 'PN' and 'O' respectively in clauses (3_P) and (3_E).

4. *Logical necessity*. It may be complained that only a very restricted sense of logical necessity has been interpreted, that the wider sense, according to which, for instance, 'No bachelor is married' is logically necessary, has been overlooked. According to the interpretations mentioned in this paper, the wider sense of logical necessity should be regarded as a kind of physical necessity. The 'physical laws' involved are what Carnap has called *meaning postulates*.

For the sake of the further discussion, let us supplement our language by the symbol of identity, '=', and accordingly add the following clause to our definition of satisfaction:

(4) $\langle D, R, f \rangle$ satisfies ' $\zeta = \eta$ ' (where ζ, η are individual signs) if and only if $v_{\langle D, R, f \rangle}(\zeta)$ is identical with $v_{\langle D, R, f \rangle}(\eta)$.

It was mentioned that Carnap has also interpreted logical necessity in such a way as to admit quantifiers. Carnap's interpretation⁷ was given in terms of state descriptions, but to facilitate comparisons I shall express it in terms of models. In the first place, we must assume that our language contains infinitely many individual constants, and we must fix a denumerably infinite domain of discourse D and a biunique designation function Des, whose domain is the class of individual constants and whose range is D. Then a *determinate extension-assignment* R is one such that $R(\zeta)$ is Des(ζ), for each individual constant ζ . A *determinate model* $\langle D_1, R_1, f_1 \rangle$ is one such that $D_1 = D$ and R_1 is a determinate extension-assignment. Let the relation C be defined as follows:

$\langle D_1, R_1, f_1 \rangle C \langle D_2, R_2, f_2 \rangle$ if and only if $D_1 = D_2 = D$, $f_1 = f_2$, and R_2 is a determinate extension-assignment.

We have, corresponding to C, the notion of satisfaction c . It can now be shown that a sentence is L-true in Carnap's Modal Functional Logic if and only if it is satisfied c by every determinate model. Carnap's interpretation is indeed suitable for certain purposes, and it can be generalized so as to apply to models which are not denumerable. But it does not permit the occurrence of two individual con-

stants naming (under certain extension-assignments) the same thing. It is just in connection with such co-extensive constants that most of the problems involving modalities arise.

It turns out that logical necessity, according to the interpretation given in this paper, is a kind of universal quantification. In fact, ' $N\Phi$ ' is logically equivalent to ' (Φ) ', where ' $()$ ' is to be replaced by a string of universal quantifiers, one for each of the descriptive constants in Φ . This quantification, though it can be expressed in a second-order predicate calculus, cannot appear in our language, because quantifiers on predicates would be required.

The consideration just mentioned will throw some light on the so-called modal paradoxes. First, does Leibniz' Law hold? In one form yes, in another form no. It can be shown that

$$(A) \quad (x) (y) (x = y \supset (\Phi_x \equiv \Phi_y))$$

is satisfied by every model, but that

$$(B) \quad c = d \supset (\Phi_c \equiv \Phi_d)$$

(where 'c' and 'd' are individual constants) does not in general hold. The dubious instances of Leibniz' Law, for example,

$$\text{Evening Star} = \text{Morning Star} \supset (N(\text{Evening Star} = \text{Evening Star}) \equiv N(\text{Evening Star} = \text{Morning Star}))$$

are indeed of form (B). But does (B) not follow from (A) simply by universal instantiation? Universal instantiation from a variable to a constant is not valid when the variable stands within the scope of 'N'. Why this should be may be seen, in a rough way, as follows: The inference in question leads, for example, from ' $(x) (\exists y) N(x = y)$ ' to ' $(\exists y) N(c = y)$ '. Let us replace, in both formulas, 'N' by universal quantifiers on descriptive constants. Then the inference leads from ' $(x) (\exists y) (x = y)$ ' to ' $(\exists y) (c) (c = y)$ '. This is clearly invalid because it violates the restrictions on universal instantiation; a free occurrence of 'x' in ' $(\exists y) (x = y)$ ' has been transformed into a bound occurrence of 'c'. A similar explanation involving a clash of quantifiers accounts for the failure of (B).

Let us consider the example of Quine which was mentioned earlier. The premise, which is true, is ' $N(\text{Evening Star} = \text{Evening Star})$ '. The conclusion, which is false, is ' $(\exists x) N(x = \text{Evening Star})$ '. The invalidity of the inference is again explained by the fact that the occurrences of 'Evening Star' involved are bound by 'N'.

Thus certain paradoxes concerned with so-called non-extensional contexts can be avoided by a method no more mysterious than rigid adherence to the rules of quantification theory. One is tempted to wonder whether other non-extensional contexts, like belief contexts, may not be susceptible to a similar analysis.⁸

5. *Physical necessity*. Let the sentences (or physical laws) of the class K relative to which we interpreted physical necessity be only finite in number. Let Ψ be their conjunction. Then it can be shown that a model satisfies 'PN Φ ' if and only if it satisfies ' $N(\Psi \supset \Phi)$ '. Hence in this case physical necessity may be expressed in terms of logical necessity. (Similarly, if the class I of ideal models is definable by a finite class of sentences, ethical obligation can also be expressed in terms of logical necessity.)

Physical necessity is closely related to the subjunctive conditional. I should like to distinguish two kinds of subjunctive conditionals, exemplified by the following:

- (1) If a meteor were to strike a planetary atmosphere at a speed greater than 100,000 m.p.h., it would burn.
- (2) If Hindemith and Stravinsky were compatriots, Hindemith would be Russian.

It is not implausible to analyze (1) in terms of physical necessity as follows:

- (3) $(x) \text{ PN}((x \text{ is a meteor} \cdot x \text{ strikes a planetary atmosphere at a speed greater than } 100,000 \text{ m.p.h.} \supset x \text{ burns}).$

It is not hard to find physical laws relative to which (3) is true.

A similar attempt in connection with (2) would yield

- (4) $\text{PN}(\text{Hindemith and Stravinsky are compatriots} \supset \text{Hindemith is Russian}),$

but this is false, if 'PN' is interpreted relative to any plausibly selected class of natural laws. What follows 'PN' in (4) seems not to be deducible from natural laws alone, but only from natural laws in conjunction with the fact that Stravinsky is Russian.

The situation is further complicated by the fact that the following sentence, as well as (2), seems to be true:

- (5) If Hindemith and Stravinsky were compatriots, Stravinsky would be German.

Yet there is something paradoxical in the joint assertion of (2) and (5).

I make the following proposal. Let the ordinary subjunctive conditional, 'If it were the case that Φ , then it would be the case that Ψ ', be regarded as elliptical, and let it be replaced by

- (6) On the evidence that X , if it were the case that Φ , then it would be the case that Ψ .

Here explicit reference is made to the evidence on which the subjunctive conditional is based. The omission of the evidence is perhaps analogous to a similar omission, pointed out by Carnap, in connection with probability₁. (6) can now be expressed in terms of physical necessity, as ' $\text{PN}(X \cdot \Phi \supset \Psi)$ '.

If (2) and (5) are expanded in accordance with this proposal, their apparent incompatibility disappears:

(2') On the evidence that Stravinsky is Russian, if Hindemith and Stravinsky were compatriots, Hindemith would be Russian.

(5') On the evidence that Hindemith is German, if Hindemith and Stravinsky were compatriots, Stravinsky would be German.

It should, perhaps, be emphasized that the interpretation given above of physical necessity (as well as of subjunctive conditionals) is relativized to a class K of natural laws. Any attempt to avoid this relativization would seem to require a characterization of the concept of a *true natural law*, but such an undertaking lies beyond the scope of the present paper.

6. *A missing law.* The familiar principle of modal logic,

- (1) ' $\text{N}\Phi \supset \Phi$ ',

is conspicuous by its absence from the system axiomatized by 1a—4b. This law clearly holds for logical necessity and quantification, and clearly fails for ethical obligation. It is perhaps not so clear that (1) also fails for physical necessity.

We must clarify what it means for a formula Ψ to *hold* for physical necessity; this will be said to occur just in case every model satisfies_p Ψ . Then if we choose Φ to be a natural law (of the class K) and consider a model which does not satisfy Φ , it is easily seen that the corresponding instance of (1) does not hold for physical necessity.

It is of course the case that all instances of (1) are *true*, in the sense of being satisfied_p by every actual model (that is, every model whose domain of discourse and extension-assignment correspond to the actual constitution of the world), on the assumption that all the natural laws in K are also true. But we are interested in the *logic* of physical necessity—hence not in those formulas which are simply true, but in those which are true in every model.

¹ The present paper was delivered before the Annual Spring Conference in Philosophy at the University of California, Los Angeles, in May, 1955. It contains no results of any great technical interest; I therefore did not initially plan to publish it. But some closely analogous, though not identical, ideas have recently been announced by Stig Kanger (in 'The Morning Star Paradox', *Theoria* vol. 23 (1957), and 'A note on quantification and modalities', *ibid.*) and by Saul Kripke (in 'A completeness theorem in modal logic', *Journal of Symbolic Logic* vol. 24 (1959)). In view of this fact, together with the possibility of stimulating further research, it now seems not wholly inappropriate to publish my early contribution.

² I use corners in the sense of Quine's *Mathematical Logic*. The interpretations given at this point are highly approximate, and can be taken literally only when an infinite domain of discourse is under consideration.

³ In fact, if one were to add the axioms ' $N\Phi \supset \Phi$ ' (where Φ is a formula of S) to 1a–4b and adopt the inference rule *modus ponens*, one would obtain exactly the theorems of S5. It should be mentioned that the schemata 1a–4b are not independent. Indeed, 3a and 3b can be derived from the other schemata, using *modus ponens*; a derivation sufficient to establish this fact may be found in M. Wajsberg, 'Ein erweiterter Klassenkalkül', *Monatshefte für Mathematik und Physik* vol. 40 (1933).

⁴ Thus satisfaction_Q is introduced by a recursive definition whose first two clauses are obtained from (1) and (2) above by replacing 'satisfies' and 'satisfy' by 'satisfies_Q' and 'satisfy_Q', and whose third and final clause is (3_Q). Similar remarks apply to satisfaction_L, satisfaction_p, and satisfaction_E, whose recursive definitions are composed of suitable variants of (1) and (2), together with (3_L), (3_p), or (3_E).

⁵ This completeness result can be stated in the following more elegant form (which permits it more easily to be compared with the principal theorem of the paper of Kripke mentioned in footnote 1). We understand by a *complete model* an ordered couple $\langle M, K \rangle$, where M is a model and K a non-empty class of models. We define recursively the relation expressed by 'the complete model $\langle M, K \rangle$ satisfies the formula Φ ', using clauses completely analogous to (1) and (2) of section 2, together with the following:

$\langle M, K \rangle$ satisfies ' $N\Phi$ ' if and only if, for every model P in K,
 $\langle P, K \rangle$ satisfies Φ .

It is then totally elementary to show the equivalence of the completeness result given in the text with the following result:

A formula Φ (of the language S) is a theorem of the system whose axioms are 1a–4b and whose only rule is *modus ponens* if and only if Φ is satisfied by every complete model.

⁶ A proof of the completeness and decidability of the system 1a–4b (with *modus ponens*) can be obtained without much difficulty from the ideas in the article of Wajsberg cited above.

⁷ Rudolf Carnap, 'Quantification and modalities', *Journal of Symbolic Logic* vol. 11 (1946).

⁸ A treatment of belief contexts, more or less along the lines adumbrated here, has since been given in Richard Montague and Donald Kalish, 'That', *Philosophical Studies* vol. 10 (1959).

DOUBTS ABOUT ORDINARY LANGUAGE IN ETHICS

by

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Many writers assume one of the major functions (if not the major function) of ethical theory is to analyze the "ordinary language" of moral discourse. This paper argues that different social groups develop quite different concepts of values; that there are many "ordinary languages." What analysts often in practice are concerned with is middle-class ethical usage. In addition, it is argued that widely accepted moral usages may be incorrect because they are based on faulty empirical generalizations, pre-scientific opinions, or socially-determined prejudices. "Ordinary language" needs to be viewed critically, therefore, rather than to be assumed as correct.

Judging from the history of philosophy, one of the major occupational hazards of the discipline is the attempt to settle by verbal means what should be settled by observation or by empirical investigation. As J. H. Randall, Jr., puts it, philosophers often do too much talking and not enough looking.¹ Many of the critics of the ordinary language approach to ethics feel that such an approach makes just this mistake.

As A. D. Woozley has observed, the notion of ordinary language is not a simple one, and departures from that language are therefore not easily determined.² There are perhaps still some who baldly assert that ordinary language is correct language, but the more important tendency holds that normally one should be cautious in departing from it, and that much bad philosophizing stems from an insufficient attention to the logic of ordinary language.³ Regardless of just how one defines 'the appeal to ordinary language,' in practice it is often assumed that such language constitutes a norm for the settling of meta-ethical disputes. Reference is constantly being made to "what we mean," or to the logic of our moral discourse, or to the usages we presumably all share.

Without question there are many areas of life in which it would be silly to take issue with ordinary language. But an excellent

case can be made that this is not so for ethics; an excellent case, that is, if one takes seriously the findings of contemporary behavioral science. Two basic questions are whether or not there is something like a standard language of moral discourse in the sense assumed by much of recent moral philosophy, and to what extent such language, insofar as it exists, is justifiable.

No great attention is normally given to the question of how extensive the use of a given ethical language actually is. Sometimes the question of non-western cultures is raised, but all too often the western view of ethics is taken as having supreme importance (or at least of being of paramount interest), and it has even been suggested that other cultures really do not have an ethics at all, if their views differ markedly from our own.⁴ Historically these attitudes probably gained impetus from the insularity exhibited by some of the earlier English analysts as well as the more modern elucidators of usages. But much more surprising is the failure, usually, even to mention the possibility that different sub-cultures or socio-economic classes of a larger culture have different modes of ethical discourse. It seems almost unbelievable that a teacher who is at all sensitive to socio-economic differences could teach, say, lower class students without becoming aware of the strikingly different view they often take of moral questions as compared to students coming from other groups. And even within a broad social grouping, there are marked individual differences which are most easily explainable as variables related to personality structure and dynamics.

It is true that these intra-cultural diversities are sometimes obscured because a dominant conception of morals prevails at least on the verbal level, and individuals become accustomed to discussing and justifying moral alternatives in terms of the dominant conception. But such a masking of genuine views is usually not at all difficult to penetrate. Surely philosophers of all people should be aware that similar or identical verbal formulations may convey the most diverse meanings. Many intelligent and well-educated American students, for example, will hold to the view that 'right' means 'what is in accord with the will of God,' even after much discussion of possible other analyses. Yet it does not require an especially perceptive teacher to see that for many of those students this definition is not in harmony with the way they make moral decisions, the way in which they judge the actions of others, or the way in which they often use the term 'right'. (From a causal point of view, the disparity between an individual's behavior

and his professed views may be a major source of egoistic, subjectivist, and emotive theories of ethics.)

The type of morality which the contemporary analysts are typically concerned with, to judge by their arguments, conclusions, and examples, is that of the intellectual middle class. Occasionally a more aristocratic flavor intrudes (perhaps this is one more "sin" attributable to Bertrand Russell), but one is impressed by the thoroughly bourgeois attitudes toward property.⁵ G. E. Moore's views as stated in *Principia Ethica* are instructive in this connection. He emphasizes legal rules for the protection of property; holds that it is most doubtful that ethics can show the utility of any rules other than those commonly followed; says that we ought never to break established rules, since we never can know when a rule should be broken; and holds that it "is undoubtedly well to punish a man, who has done an action, right in his case but generally wrong, even if his example would not be likely to have a dangerous effect."⁶ A more conservative view toward customary rules could hardly be imagined. These social attitudes fit in very well with his view of 'good' in *Principia Ethica*. How startlingly different are John Dewey's opinions about the dangers of trying to maintain old standards in a period of social change. He emphasizes the differing moral systems of socio-economic classes; calls for a constant re-evaluation of traditional moral rules; and closely relates a person's social views to his concept of the nature of morality.⁷

Surely in Dewey's case his meta-ethical theories are influenced by his beliefs about society. His insistence that value propositions rest upon scientifically verified propositions and that the same method is to be used in science and in morals parallels his conviction that social class differences are to be resolved through the method of intelligence. In Moore's case, it is at least plausible that his social outlook affected his view of 'good', although it could well be that his meta-ethical position also influenced his conservative doctrines on social questions. What is suggested in this paper is that often there is a close correlation between our socio-economic position and our meta-ethical analyses; but it is not argued that in every case, or even in most cases, the socio-economic factor is a major determinant of the approach adopted in meta-ethics.

If one goes back to earlier definitions of 'good', a sociological point of view can also be illuminating. Abraham Edel has made some interesting comments about classic Utilitarianism. Some of the peculiarities of the meaning Bentham gives to 'pleasure' can be clarified, he

suggests, if one realizes that the "properties he assigns to pleasure are the properties of money, and the reduction of all specific value qualities to pleasure is, in effect, the reduction of all aspects of human activity to the test of yielding wealth." He also suggests that Mill's introduction of intrinsic qualitative differences in pleasures can be understood partly in terms of Mill's increasing doubt about democracy and his distrust of letting the people have full political power. Mill's suspicion of complete democratic equalitarianism, then, may have been a determinant of his insistence that pleasures differ intrinsically and that the wise man's pleasures are more important than the fool's.⁸ In the case of both Mill and Bentham it seems at least arguable that their social and political commitments did affect the definitions they offered for ethical terms.⁹

However, even if the above examples are ill-chosen, substantial reasons exist against taking for granted, without critical reflection, the correctness of any particular group's view of morals. Not only has there been a long tradition in ethical theory of subjecting commonly held views to criticism, but the scientific study of human behavior seems to indicate that such widely held views are often flatly incorrect, since they are based on factual mistakes. It is a commonplace in the behavioral sciences that many moral responses are conditioned by the culture the individual lives in, the socio-economic position he occupies, his personality, etc. That this conditioning is subtle, that it takes many unusual forms; that, in short, it is not a simple process, is obvious. That it occurs, however, is also evident. While this point of view is not at all a new one, at the present time quite good scientific substantiation is available.¹⁰ By now philosophers should take seriously the fact that sometimes just what seems most sound and unexceptionable seems so precisely because of the depth of prejudice involved, the force of training, or the need for conformity of the individual.

The reply may be made that the above argument naively confuses the causal and logical orders. It may be said that one can accept the behavioral sciences' account of the influences on the objects or actions which are regarded as morally proper, and still maintain that the meanings and uses of ethical terms do not vary in any important way with culture, class, and individual. In other words, it may be argued that the logic of moral discourse is independent, or relatively independent, of the causal order which results in people making the decisions they do make.

Such a reply exhibits another traditional weakness of much philosophy; that it neglects the concrete setting in which both the language analyzed, and the philosophical analysis, occur. The Marxist insistence upon the unity of theory and practice and the instrumentalist insistence on relating technical philosophy to daily life are today much out of favor. The prevailing temper is nicely summed up in the statement of Joseph Margolis: "People at large decide what is good and beautiful and true; philosophers are content to debate what it means to say that things are good and beautiful and true."¹¹ Yet it appears plausible that often scientific knowledge of human behavior affects both our moral conceptions and the logic (if we must call it that) of ethical language. The finding of the causes for many human actions which previously were thought to be undetermined has influenced not only philosophic but popular thought on the questions of responsibility and punishment. Surely the increasingly secular views on the nature of man have been instrumental in reducing the acceptance of theological definitions for ethical terms. Not only the question of what actions are just, but the concept of justice, has been notably conditioned by political and economic changes throughout history.

Assuming that the behavioral sciences continue to progress and that a large gap remains between knowledge and common sense, a good case can be made out that widely held views on morals are likely to be incorrect, at least to some extent. Man's moral behavior is a most appropriate area for the enshrinement of outmoded views, of deeply ingrained prejudices, and of irrationality.

An example often considered in discussions of ordinary language will help to illustrate and amplify some of these points. In the fifteenth century it was not usual for people to apply the word 'round' to the world; the earth would ordinarily be given as an example of a flat object, not a round one, etc. But Columbus was factually correct and was using language properly when he rejected the accepted view. Following Chisholm, let us agree that at least part of the meaning of saying that someone uses a term correctly is that the term has the same intension for him that it has for most people.¹² In that case, a supporter of ordinary language is not embarrassed by this particular example, since the flat-worlders and the round-worlders both agreed about the intension of 'round' and 'flat' although they disagreed about the denotation of those terms. Commonly held views may be quite mistaken, therefore, without necessitating the giving up of the appeal to ordinary language. (And yet we should not forget that historically

many of the controversies in ethics have related to the denotation of ethical terms.)

If we apply this to meta-ethical problems, it is clear that some analyses of, say, 'good' which have been offered would substitute a new intension for the one many people now accept. But to show that such an attempt must be wrong would first require some evidence that the commonly accepted intension was justifiable. We should also note that a term which might have a very standard use in non-technical language may have to be substantially redefined when it becomes part of some science, and that finally the general public, or some segments of it, may substitute the scientific definition for the ordinary language one. To put the matter briefly, then, is it more fruitful to regard ethical terms as being like 'flat' or being like 'atom'?

Matters are further obscured because of the way people often come to learn to use ethical terms. Child-rearing techniques, prevailing religious views, and powerful social ideologies may all have some effect on the way we use terms. Further, the same object or action may be properly designated as good in accordance with several conflicting definitions of that term. It may also be observed that at least some people arrive at the meanings they do for ethical terms in roughly the following way. As a child they are told by authority figures that certain things are good, bad, etc. As they grow older, they may find that there is disagreement on these matters, and may shift some items from the "good" category to the "bad", or vice-versa. Eventually, if they are asked what are the meanings of those terms, their answers may be influenced by an attempt to abstract from the vast list of things they call good to some common element(s). But as noted earlier, they are likely also to be influenced by various theories they have been taught or have become acquainted with. Once a given meta-ethical theory has been accepted, they again may change their views as to whether or not some specific objects are good. In this respect, then, they may change both the intension and the denotation of ethical terms, insofar as they use those terms with any clear significance at all.

To summarize: Commonly held views which are in accord with some group's usage may be factually in error, as in the Columbus example. But knowledge of the findings of the behavioral sciences may also lead one to reject commonly held meanings or usages. And different human groups may have quite different ways of looking at the entire range of ethical phenomena. For these reasons, the appeal to ordinary language seems somewhat dubious in ethics.

This is not to deny that it may be worthwhile both philosophically and scientifically to have subtle analyses of how "we" use ethical language. But after that task is finished, there remains the further task of critically evaluating the correctness of "our" language habits. What is most sound in the appeal to ordinary language is the attempt to relate ethical theory to actual human behavior. But it would seem far more fruitful to look at human ethical behavior itself, rather than to concentrate so emphatically on linguistic usage with all of its built-in prejudices. This does not mean that ethics must be turned over to science, but rather that philosophy can ill afford to neglect the scientific data which are available.¹³

NOTES

¹ J. H. Randall, Jr., "Talking and Looking," *Proceedings and Addresses of the American Philosophical Association*, Vol. XXX, 1956—7.

² A. D. Woozley, "Ordinary Language and Common Sense," *Mind*, Vol. LXII, 1953, p. 302.

³ For interesting discussions bearing on this point, see R. M. CHISHOLM, "Philosophers and Ordinary Language," *Philosophical Review*, Vol. LX, 1951; N. Malcolm, "Philosophy for Philosophers" ("Philosophy and Ordinary Language"), *Philosophical Review*, Vol. LX, 1951; P. L. Heath, "The Appeal to Ordinary Language," *Philosophical Quarterly*, Vol. 2, 1952; G. Ryle, "Ordinary Language," *Philosophical Review*, Vol. LXII, 1953; M. Weitz, "Oxford Philosophy," *Philosophical Review*, Vol. LXII, 1953; and B. Mates, "On the Verification of Statements About Ordinary Language," *Inquiry*, Vol. 1, 1958.

⁴ For example, see D. D. Raphael, *Moral Judgement*, London, George Allen & Unwin, 1955, Ch. II; and R. B. Brandt, *Hopi Ethics: A Theoretical Analysis*, Chicago, University of Chicago, 1954, p. 9.

⁵ For an interesting account of views on property rights by a philosopher who is much concerned with sociological and economic issues, see D. C. Hodges, "Crimes Against Property," *Archives of Criminal Psychodynamics*, Vol. 2, 1957.

⁶ G. E. Moore, *Principia Ethica*, Cambridge, at the University Press, 1903, pp. 157—164.

⁷ J. Dewey, *Human Nature and Conduct*, New York, The Modern Library, 1930, pp. 81—83; J. Dewey and J. H. Tufts, *Ethics*, rev. ed., New York, Henry Holt and Co., 1932, pp. 312—13; 349—50.

⁸ A. Edel, "Context and Content in the Theory of Ideas," in R. W. Sellars, V. J. McGill, and M. Farber, eds., *Philosophy for the Future*, New York, Macmillan, 1949, pp. 431—5.

⁹ This general approach also seems to shed some light on the commentators on Utilitarianism. Thus Dewey praises Bentham for showing that "moral good . . . consists in a satisfaction of the forces of human nature, in welfare, happiness" (*Human Nature and Conduct*, pp. 211—12). Marx, on the other hand, violently opposed to Bentham's economic views, calls him "that insipid, pedantic, leather-

tongued oracle of the ordinary bourgeois intelligence of the 19th century . . . With the driest naïveté he takes the modern shopkeeper, especially the English shopkeeper, as the normal man." (*Capital*, New York, The Modern Library, 1936, p. 668). Had Marx been thinking only of Bentham's ethical naturalism, he might have been more sympathetic; had Dewey been thinking of Bentham's economics, he might have been less sympathetic.

¹⁰ This point is discussed at more length in R. Handy, "Philosophy's Neglect of the Social Sciences," *Philosophy of Science*, Vol. 25, 1958.

¹¹ J. Margolis, "On Value Theory, by Way of the Commonplace," *Philosophy and Phenomenological Research*, Vol. XVII, 1957, p. 504.

¹² Chisholm, *op. cit.*, pp. 319—20.

¹³ An excellent attempt to really make use of such scientific material is S. C. Pepper, *The Sources of Value*, Berkeley, University of California, 1958.

DISCUSSION

IS HUMPTY DUMPTY VINDICATED?

Can Humpty Dumpty be seriously taken as expounding and exemplifying a possible attitude to language? Mr. Tennessen has taken the view that this is so. It is argued here that (a) his view bears close resemblance to some recent criticisms of Wittgenstein's theory of language-games, (b) that while Tennessen's descriptive statements about the expansion of language are mostly correct, (c) his constituting this as a vindication of the Humpty Dumpty attitude is wrong. One of Tennessen's examples is analysed and his account of it is shown to be faulty.

In his paper (Inquiry, vol. 3. (1960) pp. 180—98) entitled 'Vindication of the Humpty Dumpty Attitude Towards Language' Mr. Tennessen is doing two things. He puts forward, in some detail, a theory about language which could be very briefly summarized in saying that there are no ready-made, theoretically specifiable limits of what we can say, because it may happen that that locution which looks 'unsayable', 'logically odd' etc. will be just the appropriate things to say. Then he takes this thesis to be a support, indeed a vindication, of the Humpty Dumpty attitude to language. My argument is that whilst the first of Tennessen's points is, on the whole, correct, the second is completely false.

It seems instructive that Tennessen's attack bears a very close resemblance to one which was launched recently almost simultaneously by two other philosophers. It was directed against the concept of 'language-game' in Wittgenstein's theory of language. Dr. Pole in his *The later philosophy of Wittgenstein* and Mr. Gellner in his *Words and Things* have both put forward the argument that if one accepts Wittgenstein's insistence on specifying the language-game which is being played before granting that any utterance has meaning, then one rules out as meaningless those locutions for which we have no use in our existing language-games. This, they argue, amounts to a very peculiar linguistic conservatism, which prohibits us from in-

roducing new language-games on which our intellectual progress depends. It matters little for our present purpose that the Pole-Gellner argument is historically misdirected, insofar as Wittgenstein most certainly would not have objected to the introduction of new language-games provided they were properly introduced (cf. e.g., *Philosophical Investigations* I. 18) The important point is that Pole-Gellner and now Tennesen attack a possible view of language which, presumably, was taken by some philosophers.

Tennesen, quite rightly, attempts to establish his point concerning the essential openness of language by using examples and showing how that which seems an impermissible, logically odd, nonsensical etc. mode of expression may become perfectly acceptable under certain circumstances. In Wittgensteinian terms this amounts to establishing a new language-game, in the most general sense of 'game', according to which all linguistic performances form part of *some* game. Humpty Dumpty is vindicated by Mr. Tennesen by being grouped with Russell, Strawson and Einstein, who have all initiated new language-games. I shall try to show that Humpty Dumpty does not deserve this glory.

To get down to examples. On p. 192 Mr. Tennesen says that it is possible to say '*p* and I do not believe *p*'. It has been held that to say such things is at least logically odd. Now Tennesen's point is that if there was rain in June in California, which is an unlikely occurrence, he might be justified in saying to himself while sitting in his study unaware of the rain outside 'It is raining outside while I don't believe that it is raining outside'. It is significant that he dismisses as irrelevant those 'extraordinary insights' which would clearly be necessary for the truthful and appropriate making of such a statement. As it happens, these matters are far from being irrelevant. Our language takes it for granted that we have not got those extraordinary insights, whatever they may be. To say that such a statement is a possible statement amounts to no more than that it is logically possible. But then so many impossible things are logically possible. Clearly, the example does not prove what Tennesen wants it to prove because he failed to establish the circumstances in which this statement seems to be in order. In order to justify the statement he would have had to specify just those circumstances he dismissed as irrelevant. Consequently, this substitution instance of '*p* and I do not believe *p*' remains logically odd. Of course, he might have used other substitution instances, e.g. 'He is dead and I don't believe it'. But it is doubtful whether this would

have been much better, as 'I don't' here means something like 'I don't want to'.

I have picked on the one among Tennesen's many examples which seems to me most wrong. My reason for doing this was that it shows that to render an expression permissible, logically in order etc. one has to specify the circumstances in such detail as the case requires. Once that has been done *we understand* what has been said, and this and no more is the point of specifying the circumstances. This is perfectly consistent with the practice of the late Professor Austin, who never tired of describing the smallest details of a situation in which an expression he was interested in had or could have been used. Those of Tennesen's examples which are successful are so because he has specified, with relative ease, the circumstances in which an odd statement would be appropriate. (e.g., 'I met a man today who was 360 feet tall'. p. 196).

The conclusion so far is that Tennesen's successful examples and mostly his unsuccessful example that I discussed show that in order to purify an expression from its logical oddity some new set of facts has to obtain and those facts have to be described. Can Humpty Dumpty be said to have done this? 'When I use a word it means just what I choose it to mean — neither more nor less' — thus spoke Humpty Dumpty. If this is, as I take it to be, the sum-up of his attitude then we can notice a very odd inconsistency on his part. I refer to the fact that he had to translate 'glory' as 'a nice knockdown argument' for Alice's benefit. Of course, we could say that he has introduced a new language-game. But that would be inconsistent with his previous programmatic statement because the translation somehow implies that from now on the word 'glory' is going to be used in its new sense, while the programmatic statement states that every time Humpty Dumpty uses the word 'glory', or any other word for that matter, he is entitled to give it any new meaning he fancies. Indeed, Humpty Dumpty is faced with the unpleasant choice of having to interpret himself constantly or else not being understood. We can see now how wrong Mr. Tennesen is to class Einstein for his use of 'simultaneity' with Alice's friend.

Mr. Stigen has argued (*Inquiry* vol. 3. 180-3) that Tennesen's failure to distinguish between speakers and receivers is responsible for his account of language, which implies a breakdown of communication. While I think the distinction important, I am less happy about saying that the Humpty Dumpty attitude is appropriate to

receivers. It is appropriate to nobody. But that it may seem appropriate to somebody on some occasion is due to a fairly trivial, yet often ignored, characteristic of our words. When we use a word we mean something by it, i. e. certain psychological events or experiences go with it. On the other hand, our words serve as inter-subjective counters, with a life of their own. When I apologize, saying 'I did not mean it — it just slipped out of my mouth' I bear witness to this double character of what has been said.

If Humpty Dumpty took his programme seriously he could use any word for a specific purpose as long as kept in his mind clearly what *he* means by that word, only he would not be understood. Though on second thoughts one may doubt even this. Could Humpty Dumpty *know* what *he* means unless he knew what others expect him to say? Does Humpty Dumpty have a private language? But this is another problem.

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ABSTRACTS

From "Philosophy of Science"

Vol. 27, No. 3, July 1960

Relativity and Electromagnetism: An Epistemological Appraisal, HERBERT DINGLE. This paper follows up the analysis of relativity theory begun by Margenau and Mould, by including electromagnetic theory which in their treatment was tacitly accepted. It is shown that the experiments on which Margenau and Mould rely to establish the special theory of relativity actually confirm the mutual consistency of the Maxwell-Lorentz electromagnetic theory and the special relativity theory, but throw no light on the validity of the two theories taken jointly. It is further shown that a modification of the rules of correspondence between the mathematical structure of the theories and immediate experience would bring the theories into agreement with an alternative relativity theory based on the Galilean instead of the Lorentz transformation. An experiment is suggested by which the need for such modification can be tested. A proof is then given that the rules of correspondence between the concepts of the special relativity theory (and therefore of current electromagnetic theory) and experience are not self-consistent, so that some modification of current ideas is essential. It is suggested that a generalisation of Maxwell's theory, in terms of Faraday's "ray vibrations" instead of Lorentz's static ether, might provide a satisfactory basis for a relativistic electromagnetic theory.

Universals and Particulars in a Phenomenalist ontology, ELMER KLEMKE. A phenomenalist philosophy which employs the Principle of Acquaintance (PA) plus the Principle that what exists are the referents of certain meaningful terms, defined by PA, cannot include either universals or particulars in its ontology, but is limited to *instances* of universals as constituting the range of ontological existents. Universals must be omitted since they are repeatable and, hence, never wholly presented or contained, whereas the objects of direct acquaintance are wholly and exhaustively presented. Furthermore, no entities beyond characters (qualities and relations) are given in direct acquaintance; hence, particulars, too, must be omitted for the phenomenalist who relies on PA.

The place of induction in science, MARIO BUNGE. The place of induction in the framing and test of scientific hypotheses is investigated. The meaning of 'induction' is first equated with generalization on the basis of case examination. Two kinds of induction are then distinguished: the inference of generals from particulars (first degree induction), and the generalization of generalizations (second degree induction). Induction is claimed to play a role in the framing of modest empirical generalizations and in the extension of every sort of generalizations—not however in the invention of high-level hypotheses containing theoretical predicates. It is maintained, on the other hand, that induction by enumeration is essential in the empirical test of the lowest-level consequences of scientific theories, since it occurs in

the drawing of "conclusions" from the examination of empirical evidence. But it is also held that the empirical test is insufficient, and must be supplemented with theorification, or the expansion of isolated hypotheses into theories. Refutation is not viewed as a substitute for confirmation but as its complement, since the very notion of unfavorable case is meaningful only in connection with the concept of positive instance. Although the existence of an inductive method is disclaimed, it is maintained that the various patterns of plausible reasoning (inductive inference included) are worth being investigated. It is concluded that scientific research follows neither the advice of inductivism nor the injunction of deductivism, but takes a middle course in which induction is instrumental both heuristically and methodologically, although the over-all pattern of research is hypothetico-deductive.

Pragmatism and the ideal language, L. E. PALMIERI. Pursuing a line of progression articulated by Prof. Quine, Dr. Pasch (*Experience and the Analytic*) argues that the analytic-synthetic distinction rests on mere convention. Further, that the use of this distinction by present day empiricists—especially the rational reconstructionists—has caused empiricism to take a departure from *traditional* empiricism. I observe, in opposition, 1) the natural language firmament is itself an amorphous construct, 2) the natural language might be the language of experience but not of empiricism, 3) the ideal language is tied to experience by primitives for what perceptually appears, and 4) if one claims the primitives must be tested in scientific inquiry, a case should be made for this *philosophical* position.

On a proposed revolution in logic, HECTOR NERI CASTANEDA. In his *The Uses of Argument* (Cambridge University Press, 1958), S. Toulmin presents serious charges against ordinary logical theory, e.g., that it does not distinguish between analytic or formally valid or conclusive or warrant-using arguments, that the distinction between premises and conclusion is a bad oversimplification, that "major premise" conceals the distinction between inference-warrant and inference-backings, that logicians have been mistakenly working under an ideal of geometrical form.

The paper argues that none of the charges is proven, that most of them cannot be proven, that Toulmin's new logic is at best only vaguely hinted at and that his suggestions are positively obscure or mistaken.

Confirming power of observations metricized for decisions among hypotheses, HENRY A. FINCH. Part I.

Experimental observations are often taken in order to assist in making a choice between relevant hypotheses $\sim H$ and H . The *power of observations* in this decision is here metrically defined by information-theoretic concepts and Bayes' theorem. The exact (or maximum power) of a new observation to increase or decrease $\Pr(H)$ the prior probability that H is true; the power of that observation to modify the total amount of uncertainty involved in the choice between $\sim H$ and H ; the power of a new observation to reduce uncertainty toward the ideal amount, zero, all these powers are systematically shown to be exact metrical functions of

$$\Pr(H) \text{ and } \frac{\Pr(o/H)}{\Pr(o)} - 1$$

where the numerator is the likelihood of the new observation given H , and the denominator is the "expectedness" of the observation.

Panpsychism or evolutionary materialism, ROY WOOD SELLARS. I shall be concerned in this paper with the consideration of panpsychism and of materialism in new forms as alternatives. Extended reference will be made to C. S. Peirce's view of perception as realistic in intention and yet not quite clear as to its mechanism and how it attains objective import. I shall say little about Whitehead as a representative of panpsychism as I have just finished a detailed criticism of his epistemological framework. I shall, however, make comments on William James's radical empiricism as tied in with his view of perception as direct and immediate — roughly speaking, the alternative to Locke's representation of "unperceived things" — and bring in my own theory of sensations as guiding perceiving. Russell's neutral monism, connected historically with James's radical empiricism, will be touched on here in connection with his rejection of materialism. Phenomenalism and materialism exclude each other. Materialism, as an ontology, requires a realistic epistemology. I shall also make some comments on Dewey's *biological experientialism*. One can often best explain a perspective by means of contrasts.

Is the philosophy of science scientific? A. CORNELIUS BENJAMIN. It is helpful for any enterprise to stop occasionally and examine itself. Science has done this rather infrequently in its long and eventful history, and there has not been, in general, any continuity in these self-examinations. As a result the history of the philosophy of science has been a rather spotty affair. My belief is that the philosophy of science should also, at times, become self-critical. When a study is concerned primarily with methods of other disciplines it tends to underemphasize the role played by the methods which it itself uses. What is the method of the philosophy of science, and is it justifiable? Can there be a satisfactory study of science and can this be scientific?

Studies in the empiricist theory of scientific meaning, WILLIAM W. ROZEBOOM. *Part I — Empirical realism and classical semantics: a parting of the ways.* *Part II — On the equivalence of scientific theories.* Part I is concerned with the tenet of modern Empirical Realism that while the theoretical concepts employed in science obtain their meanings entirely from the connections their usage establishes with the data language, the referents of such terms may be "unobservables", that is, entities which cannot be discussed within the data language alone. Such a view avoids both the restrictive excesses of logical positivism and the epistemic laxity of transcendentalism; however, it also necessitates a break with classical semantics, for it follows from the empirical realistic position that a theoretical term must in principle be capable of simultaneously designating not just one entity, but indefinitely many.

Drawing upon the Carnapian explication of "analytic truth", Part II examines a possible axiomatic basis for the empiricist theory of scientific meaningfulness to demonstrate that even if theoretical terms are able to designate entities inaccessible to the observation language, as held by Empirical Realism, so long as the meanings of theoretical terms derive from their connections with the observation language, the meaning content of a theory is exhausted by its observational consequences.

Imperatives, logic, and moral obligation, ROBERT G. TURNBULL. It is claimed that 'Do x !' means 'Then you will do x '. Answering a "Why?" question concerning the former

may take either of two forms, viz., 'Because ...' or 'If you wish to ...' The second answer completes the truncated hypothetical. "Ought" sentences are treated as a species of imperatives involving universality in the "if" clause ('If *anyone* wished to ...'). Moral "ought" sentences involve a *double* universality, viz., the one mentioned above *and* universality connecting the action with social harmony (e.g., 'If everyone were to do x , then there would be social harmony').

Confirming power of observations metricized for decisions among hypotheses, HENRY A. FINCH.
Part II. No Abstract.

Concerning Mr. Feigl's "Vindication" of Induction, Daniel Kading. No Abstract.

